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The language of science

A distinguishing characteristic of scientific knowledge is the precision with which it can be communicated. In this respect it approaches the level of mathematics and far surpasses that of any other branch of learning. This is due in part to its subject matter, but in part also to the care with which men of science have gradually learned to frame definitions and connotations. Such carefulness did not always exist; it grew with the growth of science, and each afforded stimulus to the other.

One of the difficulties of early scientific terminology arose from its polyglot vocabulary. Most European, and some Asiatic, peoples of the ancient and mediaeval world made their contributions to scientific knowledge, while in Africa the Egyptians provided full justification for the Greek proverb reported by Pliny: *Ex Africa semper aliquid novi*. When scientific facts were borrowed from foreign sources, the foreign terms were frequently taken over as well, and continued repetition of this process led to a vocabulary of extreme heterogeneity.

To gain some idea of the extent to which appropriation of exotic names was carried, it will suffice to glance at the terminology of a single branch of science, namely chemistry. The chemist's debt to Latin and Greek is so obvious as to make comment superfluous; but he drew sulphur and anil ultimately from Sanskrit; marcasite and sapphire from the Assyrian; cinnabar, laudanum, musk, naphtha, from the Persian; alcohol, alembic, benzoin, realgar, sugar, tartar, turpeth, and a host of other words from Arabic; and the name of his science very possibly from the land of the Pharaohs. The vocabulary was still further enriched—if that is the proper word—by the adoption of cryptic and fanciful names for chemical substances. Equation of the seven metals of the ancient world to the sun, moon, and planets (silver nitrate is even yet 'lunar caustic') was intelligible enough; but such *Decknamen* as 'the red dragon' (nitric acid), 'the black crow' (lead), and 'the white eagle' (ammonium chloride), inevitably added to the difficulty of transmitting information accurately, more particularly when, as often happened, the same name was applied to two or more different substances.

Similar confusion and lack of precision marked the nomenclatures of other branches of science, especially the biological ones. In botany the position was rendered worse by the universal acceptance of the 'doctrine of signatures,' according to which the Deity had not only stamped upon plants a distinct form, but had also 'given them particular signatures, whereby a man may read even in legible characters the use of them.' Various interpretations of the 'signatures' could clearly lead to false identifications, as occurred, for example, in the case of moonwort (*Botrychium lunaria*) and honesty (*Lunaria biennis*).

With the advance of knowledge, it at length became an urgent necessity to define and codify scientific terms, names, and descriptions. Among the pioneers in this essential labour was Boyle (1627–91), whose definition of what he conceived to be the nature of a chemical element leaves nothing to be desired. Thirteen years after the death of Boyle, another British pioneer, John Harris, broke fresh and fruitful ground by the publication of the first technical dictionary ever compiled in any language. Harris and his work form the subject of an article by Dr McKie which we publish in the present issue.

Half a century or so later, Linnaeus (1707–78) introduced a great measure of precision into botanical nomenclature and terminology, more especially by his system of binomial definition as expounded in the *Systema Naturae*. This system, according to which a plant is defined by two

Latin or latinized names—the generic name and the specific name—proved of incalculable assistance in botanical progress; and its adoption in zoology was equally beneficial.

The work of Linnaeus in clarifying the language of biology was shortly followed by a fundamental reform in the nomenclature of chemistry. A small committee of French chemists—Lavoisier, Guyton de Morveau, Berthollet, and Fourcroy—in 1787 published a report which, after pointing out the absurd and misleading nature of such names as 'butter of antimony,' 'liver of sulphur,' and 'sugar of lead,' set forth for the first time the essentials of our present system of chemical names. The scale of the improvement may be sufficiently gauged from a single example: the substance that Lavoisier and his collaborators proposed to call 'carbonate of potash' was previously known by no fewer than eight different names, all of them uninformative—fixed salt of tartar, fixed vegetable alkali, aërated fixed vegetable alkali, tartar of chalk, mephitic tartar, mephitic potash, self-fixed nitre, and van Helmont's alkahest!

The principle of the French committee, that the name should exactly express the idea, and the idea exactly describe the fact, is the underlying principle of organic chemical nomenclature. The names of organic substances are, in effect, expansions of their formulae, so that formulae may be deduced from names and names from formulae—by those who know the rules. The convenience, indeed the necessity, of such a scheme is manifest; the furtherance of chemical knowledge would now be impossible without it.

Chemical nomenclature is only one aspect of the general problem presented by the language of science. Precision of knowledge, as we have seen, demands precision of expression, but this in turn can too easily develop into a narrow formalism which renders it obscure except to the specialist. Take, for instance, the following sentence chosen at random from a recent book on biochemistry: 'The nucleate quarter gave viable parthenogenetic gyno-merogons . . . ', and consider the amount of exact information it conveys to the biochemist—but how little to the physical chemist or physicist. Yet that information would be perfectly intelligible if expressed in less formal language.

When specialist terminology is skilfully employed, it has, for the initiated, the clarity and brevity of mathematical symbolism, but examination of scientific textbooks and articles quickly shows how much it lends itself to abuse. Here is another quotation, this time from a book on radio: 'With choke capacity coupling an iron cored choke is used in place of the anode resistance used in resistance capacity coupling.' That kind of writing is indefensible on any standard, but unfortunately it is all too common. Sometimes, perhaps, it betokens nothing more serious than a lack of literary sensibility, but lucid thought should not have difficulty in arriving at lucid expression; when, therefore, the expression is confused, there is more than a probability that the underlying thought is confused. It is unfortunate that many scientists, especially among the younger ones, affect to misprize any style of writing which aims at the imagination or paints a word picture. Such writing they dismiss as jargon, when in fact it is they themselves who write an obscure and colourless jargon, strung together of technical terms and well-worn *clichés*.

Men of science have a double concern with words. We have indicated one of them, in the emphasis on the value to science of a precise terminology. The other concern arises from the increasingly arcane character of this terminology to all but the adept. Wide appreciation among the general public of scientific aims, methods, and results is an urgent necessity, both for public welfare and for the advance of science. A highly technical language, while indispensable for scientific progress, strongly militates against the lay comprehension of science, and the position is thus not an easy one. A favourable factor, of which insufficient advantage has so far been taken, is that very much scientific knowledge would be within the lay understanding were it expressed in less professional language. Such expression would naturally lack something in precision and conciseness, and would make heavy demands upon the time and energies of scientists. But the effort is not merely desirable: it is an ineluctable labour if the already formidable gap between the world of science and the world at large is to be successfully bridged.

Contributions and correspondence should be sent to the editor: E. J. Holmyard, M.A., M.Sc., D.Litt., F.R.I.C., Imperial Chemical Industries, Nobel House, Buckingham Gate, London, S.W.1. Scientists engaged in research of an interesting or important character are invited to send short notes on work in progress and results obtained.

Some growing-points in experimental psychology

F. C. BARTLETT

'The proper study of mankind is man,' but it is an exceedingly complex and difficult study. In any item of individual human behaviour so many and such diverse factors are involved that the application of scientific method to their disentanglement might appear hopeless. Professor Bartlett shows that, on the contrary, a good beginning has been made, and that, under the stimulus of war, much accurate information has been obtained about the determination of everyday behaviour. The field of theoretical study thus opened up is large.

I. BEGINNINGS

Experimental psychology has had a short but crowded history. The University of Cambridge might have secured for England the honour of establishing the first laboratory for its study, for in 1877 Professor James Ward and Dr Venn proposed the founding there of a department of psychophysics. But the attempt was defeated, and the credit passed to Wilhelm Wundt who, two years later, opened the first laboratory of experimental psychology in Leipzig. Wundt was more of a systematizer than an experimentalist, and, for all his enthusiasm and his immense authority, he was not a particularly original one either. He was much more interested in the description of human experience and of the structure of human knowledge than in the direct and controlled study of special forms of human behaviour. Influenced, perhaps, by contemporary chemical research, he sought for the irreducible elements in man's knowledge of his environment. He found them, as others had done, more by logic than by experiment, in sensation and simple feelings. He then set himself to describe and name all the 'compounds' into which these elements could enter, and all the forms of connexion which mature experience had brought about between the 'compounds.' Experiments were extensively used, but on the whole they were used to illustrate the theory rather than to discover the facts.

Wundt exploited and built upon the great amount of experimental study of human reactions which, in a more sporadic way, had preceded him. This was almost entirely the work of physicists and physiologists. His programme for the psychologist required an initial study of much anatomical detail, and on the experimental side proper the investigation of three main fields. These were: the correlation of measurable characters of physical stimuli with the 'qualities of sensation,' especially those of 'mode' and of intensity; the detailed exploration of the reactions of the local mechanisms in the human body, particularly of the special senses; and the measurement of the timing of extremely simple sensorially determined movements. To these he added many further experiments designed to demonstrate the nature of the universal categories of space and time and their place in human knowledge.

Wundt deserves all the renown which he achieved, but he gave experimental psychology a disastrous twist towards formal theorizing and all-embracing explanation, from which it is only just beginning to break free. It was very natural that within a few years other investigators should appear, with other elements, different compounds, and divergent forms of combination. The field was set for a prolonged wrangle between conflicting 'schools' and opposing general theories.

Yet, even while Wundt was consolidating his position as a sort of dictator in the psychological world, another German, Hermann Ebbinghaus, was patiently experimenting upon his own memory processes. This was a tremendous stride forward in two ways. To begin with, memory is an example of what are often called the 'higher mental processes.' It has no direct sensory conditions at all. The field of experiment in psychology was being immeasurably broadened. But more important still, Ebbinghaus was interested in how his memory worked, rather than in what his memory was, or in any description of its precise place in a structure of knowledge about the world. His outlook was genuinely functional, and hence both his methods and his problems were essentially experimental. He found out how to prescribe and arrange the conditions he needed, how to change them, and how to note, record, and measure the

resulting change in memory behaviour. In spite of a certain artificiality which hangs over his work, he elucidated a number of principles about the course of remembering and forgetting; about the relation of the amount of material learned and retained to the amount and method of initial memorization; about the effects of prompting; and about the relative weight and frequency of particular types of connexion formed between items of material presented, which have triumphantly withstood the attacks and results of all subsequent criticism and experimentation.

The conflict between a structural and a functional point of view, which broke out very quickly after experimental psychology had received full recognition as a special branch of exact study, has continued to the present day. So far as experiment is concerned, however, its issue was certain from the first. Nobody doubts now that when a man uses experiment in psychology he sets out to discover the conditions of a particular kind of behaviour, or function. It may be of some special sense; it may be of those mechanisms which promote any sort of bodily movement; it may be of those very much more complicated, but not less exactly determined, mechanisms which issue in some movement of the mind. With differences in accord with his subject-matter he is in exactly the same position as the experimenter in any other branch of natural knowledge. When he has found out the conditions of the behaviour in which he is interested, and has measured the results of specific changes of conditions in whatever way may be possible and appropriate, there is no obligation upon him to use his results in order to arrive at some theory of the whole of human experience or knowledge. If he does this he knows well that he is going beyond the limits of his experiments.

I have said that a certain artificiality marked the early remembering work of Ebbinghaus. Since consideration of this artificiality leads directly to one of the most interesting of the growing-points of contemporary method in experimental psychology, it is worth looking into the matter with some care. Ebbinghaus was a first-rate experimenter. It was therefore natural and right for him to consider that anybody who first sets out to study any given function must take the utmost care to isolate that function, so that he can see it in action, uncontaminated by unknown and probably incalculable relations with other functions. In the particular case of memory the most obvious difficulty is to get every item of material to be memorized equivalent to every other item, and to do this without repeating any item exactly. If the language, or the visual forms, or the movements, familiar in everyday life are used, every person who comes to the experimental situation will have a different and unknown handicap compared with any other person. The result will express the operation of remembering *plus* intelligence, or special facility, or special training, or something of that sort; but never of memory alone.

Ebbinghaus hit upon the simple but nevertheless brilliant device of using symbols which, he thought, could have no *particular* meaning for anybody. All the other conditions of memorizing: mode of presentation, number of times repeated, number of items used, and so on, could be perfectly well controlled, and the result ought to show what remembering itself can do when nothing else affects retention and recall. It soon appeared that the case is not so straightforward as this, and that even the most apparently meaningless symbols can and do frequently set up a host of associations which may easily produce uncontrolled differential bias in different people and on different occasions. Ways were found, however, of dealing with all these difficulties, and in the end the classical early experiments did in fact achieve the desired aim of setting memory to work in at least relative independence of related human functions.

Beyond question, this principle of isolating reactions and functions is both necessary and vital, especially in the early stages of experimental development. If, however, it is maintained rigidly and without limit there may come a time when it issues chiefly in the accumulation of more and more detail all of which may mean less and less in relation to the main topic of study. This is particularly the case in a subject like psychology, where different functions are closely interconnected and one may play upon another in an intricate manner. For example, it is easy enough to show that when normal reaction-times are reduced to their simplest elements, isolated, and measured for a number of age groups, the reaction-times begin to lengthen by a few thousandths of a second in the early 30's and go on lengthening as age increases. Yet W. R. Miles [1] was able to show that when visual reaction-time is measured in a setting of snap-shooting—that is in relation to accompanying functions combined in a form of skill—such lengthening need not occur at all. Or again, as will appear later, in other cases simple reaction-times may show no

appreciable variation, whereas the timing of concerted action involving exactly the same elements may vary significantly.

Another case, which is both of theoretical interest and of great practical importance, is to be found in the experimental study of fatigue. The accepted method has been almost always to isolate some simple reaction and to repeat it under controlled conditions until it begins to fall off in measurable amount or efficiency. Under these circumstances, in a very large number of instances nothing much happens for a relatively long time, and then comes a very sudden and catastrophic fall of apparent efficiency. But if the separate reactions are linked together to form a skill, and the skill itself is measured, or its course plotted, a very different story may have to be told. Before—sometimes long before—the individual components of the skill have reached a stage of deterioration, the internal timing or spacing of the components begins to suffer, and the performance tends to go by fits and starts, worsening and then picking up again. Some of the constituents are liable to get forgotten or crowded out. Stop the skill, test the individual constituents, and the operator may appear as perfect as he was at the beginning. In fact, however, the performance has changed, because the relationships of the constituent reactions have become persistently different from what they were in an early stage of the work.

The time is now ripe for a major effort on the part of experimental psychologists to discover reliable ways of studying activities which are contemporaneously conditioned by stimuli of different modes. This is indeed beginning to be tried, and to some of its illustrations I propose now to turn. Further, such activities must be considered, not as single and isolated items of response but as events closely interconnected in a chain of behaviour.

II. HOW EVERYDAY BEHAVIOUR IS DETERMINED

(*a*) *The Linking of Stimuli*

In any survey of the development of scientific research it has always been difficult to draw a hard line of distinction between the solution of problems in the interests of theory alone, and the advances which have sprung from efforts to surmount practical difficulties. This is particularly true in psychology, whose field is the whole range of human behaviour. The chief stimulus to the devising of experiments has almost always come from beyond experimental situations themselves, from the difficulties which men have found in dealing with the rapidly changing conditions of day to day environment. It seems fair to say that the periods of genuine scientific growth in psychology have synchronized with periods in which men have had to solve intensely practical problems, and that in this field especially the necessity for applying knowledge has been the main spur to its advance.

The increasing speed of modern life, fast outrunning any known resources of education and training, piled up industrial and transport accidents and forced psychologists to give concentrated attention to the essential relationships of sensory cues and bodily movements. From this developed new methods for detecting failures of bodily control under stress, and a whole realm of theory about 'accident proneness,' the complete significance of which is as yet far from fully appreciated.

Intense growth of specialization in every sphere of life, and a simultaneous world-wide economic competition, were very strong influences—though not the only ones—in leading psychologists to look for more reliable and valid methods of determining high and low levels of intelligence, and for detecting the basic qualities needed for the development of special skills. This has led to large extensions of theory about the foundation of intelligence and its character, and about the varieties of particular skills and the proper ways by which they can be trained.

The growing-points which I want particularly to consider have almost all been a result of the all-pervading mechanization of human effort, and the present war has been their occasion. When the war broke out, experimental psychologists all over the world were asked questions about how men could be expected to behave in particular situations, usually under stress, and generally with machines to help or hinder them. Every psychologist must have been surprised and somewhat humiliated to find that in spite of the immense bulk of laboratory investigation of the past sixty years there were very few of those questions that he could decisively answer. The primary reasons were almost always the same. Much, though not enough, was known about the action of particular senses in response to particular stimuli, but little about how different senses and different perceptual processes collaborate in response to varied stimuli of diverse mode. The normal course and timing of simple movements

had been established. Far less was known about their limiting conditions of accuracy, and almost nothing about their serial interconnection in continued activity.

Every machine sets its operator two types of problem. First, it provides certain stimuli which he must use as cues for action. These stimuli are rarely static; usually they change in a more or less predetermined manner. Second, it provides him with wheels, levers, switches, or other devices which his hands, feet, and maybe other parts of his body must adjust in accordance with the changing stimuli to produce desired results. The adjustments are almost never single events, but serial.

It is convenient to use current terminology, and to speak of all problems of the first type as problems of display, and all those of the second type as problems of control.

The majority of machines are built on the assumption—probably correct—that the easiest and most definite sort of display for a normal man to deal with, is visual. But it is not common to find an operation depending upon visual display alone. Whether by set purpose or as a result of experience the visual cues will be supplemented by others, perhaps auditory or tactual, perhaps of taste and smell. Movements of control inevitably set up still further sensory stimuli which, though they are secondary in origin, may come to have enormous determining influence, and are truly a part of the display.

Sometimes the different sensory stimuli which are present at the same time prompt the operator to make opposed movements, or to try adjustments which can be fitted together only with difficulty. This, for example, is very likely to happen when the whole machine is mobile. Bodily posture, the orientation of the machine itself, the speed and direction of movement, may all seem different according to whether the operator relies upon his bodily sensations, or upon his reading of recording instruments like those on an aircraft panel. If the man obeys his own movement and posture sensations he will do one thing, if he follows his visual sensations he will do a different thing.

Only after this situation had become a suspected source of serious difficulties, especially in flying, was it very carefully studied. It is of course easy enough to set up the required conditions experimentally. Some interesting results were found. First, the dominance of visual cues, in cases of 'sensory conflict,' is not nearly as great as is commonly supposed. Second, most people have a natural bias towards some particular mode of sensory reaction which will often persist even though great pains may be taken to overcome it, and which will tend to return under conditions of stress or fatigue. Third, the number of people who try to make 'compromise' responses under these conditions, most of which will run them into difficulty, is considerable. Fourth, the effectiveness of particular sensorial stimuli may be very strongly influenced temporarily by various recent or accompanying experiences, many of which may not themselves be sensorial [2].

Each of these results opens up a wide field for experiment. For various reasons, the one relating to stress and fatigue seemed to be the most interesting and urgent. Why should work under pressure apparently revive some reaction which seems to have been 'trained out' of the operator, and which he certainly does not wish to make? To answer this and related questions a way had to be found to study 'skill' fatigue, or work deterioration characteristic of continued co-ordinated response adapted to a changing pattern of stimulation when more than one mode of stimulus is active. Since, once again, the most prominent practical situation implicated was that of the aircraft pilot, an experimental cockpit was built in which the essential constituents of behaviour of the pilot in flying could be reproduced and graphically measured. It soon became clear that 'skill' fatigue and straight 'muscular' or simple 'sensory' fatigue are not the same thing. What particularly characterizes the former is, except in extreme cases, not a growing inability to perform the constituent items of the skill, but a failure to time these reactions properly. It is less likely that the wrong thing will be done, more likely either that the 'right' thing will be done at the wrong time, or that something which is practically irrelevant will be done [3].

The experimental cockpit has shown that it is possible to investigate the conditions and course of a highly elaborate skill in a controlled manner and with objective records. Obviously, however, it cannot be widely used, for it requires a very specially and carefully trained operator. Some simpler situation must be found which nevertheless preserves the essential characters of skilled behaviour. This is much less easy than may appear, and a fully satisfactory experimental set-up has not yet been achieved. Figure 1 illustrates what is probably the best attempt so far made. As shown, the operator turns a wheel with his left hand and by this, with a form of velocity control, directs the movement of the vertical

pointer in relation to the three lines marked on the lower white screen. Above this screen are two lights. If the one to the left is flashed the operator must move the pointer on to the line on the left. A light on the right means a similar but opposite movement. Both lights may appear together. Then the operator must produce a movement in the direction of the brighter signal. The lights may be near or below threshold difference, and in this case the operator is faced with a dilemma. At the same time the lever to the right must be held against some predetermined resistance so as to keep the pointer on the upper white screen in a steady position. All movements made are recorded and so are the reaction-times of each item of response.

Figure 2 shows, in rather simplified form, three characteristic behaviour records. The first is in all respects normal, the second depicts a special type of disturbance after a 'difficult' stimulus, while the third is that of an operator who was suffering from a great degree of cumulative fatigue. The first shows a perfectly straightforward response to a single, i.e. a 'one light,' stimulus: both original deflection and return are made without block and with a minimum of 'wobble.' The difficult, i.e. critical 'two light,' response is made well, but the significant fact is that *after* it has been accomplished there is a fairly long continued 'wobble' which affects the return and prevents the operator from holding steadily the position desired. A more pronounced type of after-effect of a 'difficult' reaction appears in the second record.

The third record at first glance seems to show a greatly reduced activity. Certainly the visual stimuli give rise to smaller and more spread-out reactions; but overall there is heightened activity. There is a pronounced 'wobble,' with over- and under-shooting—especially after the return—even following the simple stimulus, and more still, though the section of record given does not pursue this, after the difficult stimulus. Moreover, the

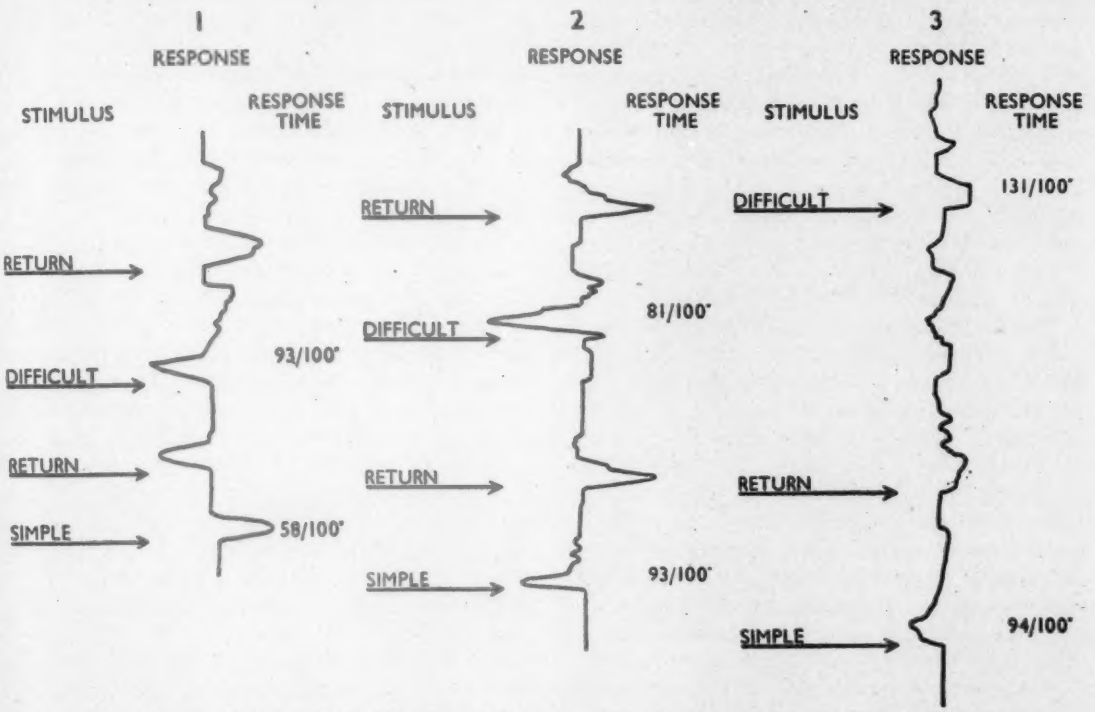

FIGURE 2 – *The graphic record* 1, *which was of the movements of the control, shows a nearly perfect response to a simple stimulus and a return stroke. Both the response and the return after a stimulus requiring a difficult discrimination were disturbed, the outward movement being made less quickly and the inward movement being less regular. Record* 2 *shows another type of disturbance. The first movement was made in one direction after a short response time and was then reversed. The subsequent outward movement was of a greater extent than that to the simple stimulus.*
Record 3 *was obtained from a 'fatigued' subject. There were very marked restless movements between the stimuli of almost as great an extent as the responses themselves. These were small and irregular, and the response times were unusually long.*

reaction-times recorded showed a significant slowing, especially in the difficult cases; though when these times are recorded alone, and in a separate experiment, they frequently show no difference whatever between fatigued or 'cautious' operators and any others.

The effect of the simultaneous manipulation of the right-hand lever cannot be shown here. There is, however, a strong suggestion that, especially when the resistance to be overcome is fairly considerable, the left hand then moves with greater vigour and a consequent increase of over- and under-shooting.

Enough perhaps has now been said to show that experiments based upon situations in which stimuli of differing mode and significance are simultaneously present raise some extraordinarily interesting issues. Their development is likely to yield results bearing upon mechanical operations of many kinds, whether highly specialized, as in piloting an aircraft or tracking a target, or in routine work in industry. The design of displays and control in accordance with widely distributed human functions, always a matter of importance, will be set upon a sound scientific foundation. Apart from this, a large field of theoretical study is opened up, for the prevalent notion that the course, character, and conditions of co-ordinated human functions can be safely deduced, if only the outstanding features of their constituent elements are known, is shown to be unsound [4].

(*b*) *The Linking of Reactions*

Very little human behaviour consists of a single simple reaction or movement. The same movements may have to be repeated in a series, or a cycle of movements may be repeated, or different movements must be suitably spaced in sequence one to another. Yet when experimental psychologists and others have investigated reaction-times they have usually considered only the interval between the display of some very simple stimulus or group of stimuli, and some equally simple prearranged response, such as making or breaking a circuit by a small movement of finger, hand, foot, lips, or the vocal organs.

Moreover, the time indicated has very nearly always been that of the single reaction, whether to one unvarying form of stimulus, or to one in a group whose character must be 'discriminated,' or whose specially arranged response must be 'chosen.' Considering the great amounts of ingenuity and work that have been expended on the measure of reaction-times, the results achieved have been disappointing, whether from a theoretical or from a purely practical point of view.

Yet it has become very clear that the timing of items of reaction *in relation to preceding and following items* is a matter of first-rate importance. The great need, now beginning to be met, is to see the individual reaction-time as an incident in a sequence, not independent of its place in that sequence, but varying in some probably constant manner because of what precedes and is likely to follow it.

Some attempt has already been made to reanimate the study of reaction-times in this sense. Visual accommodation-times have been measured in series, when the display consists of a C, appearing alternately near and far, with the gap to N., S., E., or W. The subject records the seen position of the gap with a simple finger switch. When he is working at pressure, that is, above the rate at which he makes only very occasional errors, 'breakdowns' are recorded. A 'breakdown' is an empirically determined consecutive number of 'wrong' readings or of omissions. If we take the average reaction-time of any six consecutive correct responses preceding a 'breakdown,' we find that there is a consistent lengthening, and that the increase immediately before the breakdown point is significant. Whether this is due to an increase of accommodation-time only, or whether the finger movements and the visual receptor processes also play a part, is not known.

The interesting thing is the demonstration that the time of a reaction may be much affected by the place of that reaction in the series of which it is one item. This is, of course, exactly what would be expected on general grounds, and only a great bias towards dealing with isolated items can have prevented reaction-times from being investigated with more relation to serial factors in their determination [5].

(*c*) *The Accuracy of Control of Bodily Movements*

Experimental psychology may thus be expected to give more and more attention, in the immediate future, to a study of human skill, its conditions of establishment, its basic constituents (especially on the side of bodily movements), and perhaps particularly to the timing, both of the constituents in relation to their place in the whole performance and of the whole performance itself. But it is impossible to go far with these studies without a serious attempt to determine the limits and conditions of accuracy of the most fundamental simple types of movement involved in skill. Once again,

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FIGURE 1 (*left*) – *The subject causes the pointer to traverse across the display by moving the wheel control, which acts through an integrating disc mechanism (to be seen on the left of the picture). The pointer is held on the middle line of the display until a light signal appears which indicates to which of the side lines the pointer is to be moved. The subject brings the pointer back to the middle line on a verbal command and awaits another light signal. The lever to the right must throughout be held against some predetermined resistance so as to keep the pointer on the upper white screen in a steady position.*

FIGURE 3 (*below*) – *Muscle Movement Test. The operator must try, by a movement controlled by a hand and wrist, to move the pointer from the lower line on the drum to the upper without seeing what he has done. After moving the pointer back, he can look at the record and try again. Repetition gives him the 'feel' of the gear-ratio, and he settles down to a steady performance. Relatively small errors occur when the amount of movement required lies between 30 and 250 mm., but with smaller movements the margin of error increases rapidly.*

FIGURE 5 (*below*) – '*Jumping monkey.*'

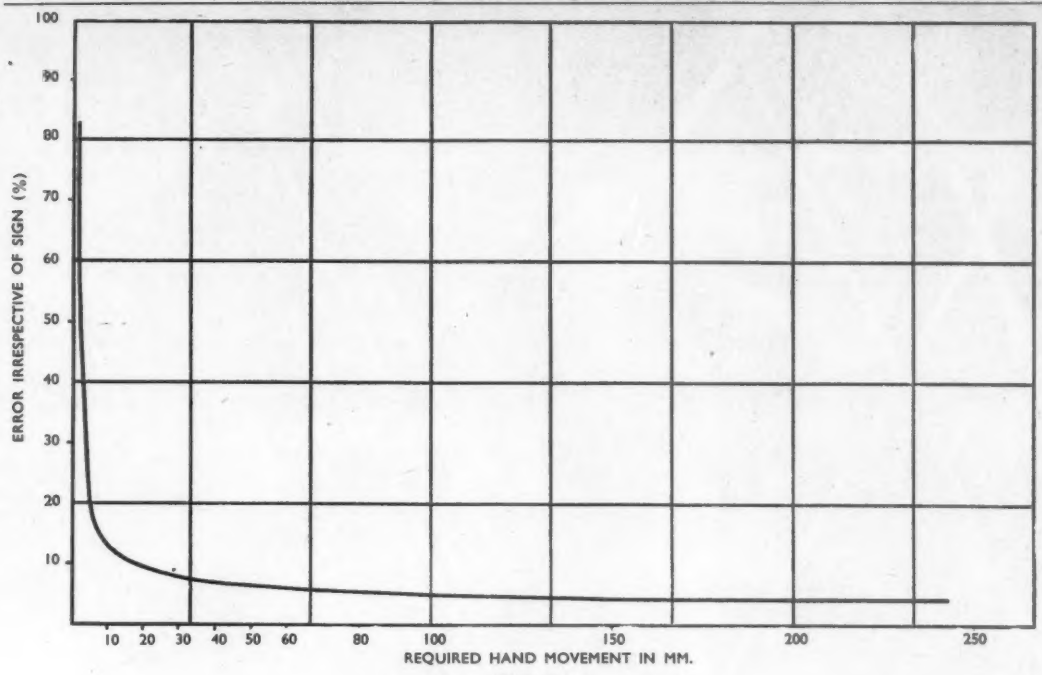


FIGURE 4

it is practical needs which have forced a new approach to this old problem, and these needs have grown out of that same process of replacing the human worker by the machine which has already been mentioned.

Machines can do much, but usually need minders; and minding a machine may require skill, often of a high order. There may, however, be periods during which all the minding required is itself within the range of what machines can do. The 'robot' pilot, or other form of 'servo-mechanism,' can give the human pilot, or other machine-minder, relief from strain; but during critical phases of a flight, when things are going to happen at a high rate of speed, or in an unexpected manner, the human pilot takes over, and the same kind of thing happens in other fields.

It looks as if the greater adaptability and flexibility possessed by the human operator have both disadvantages and advantages. What are they, and how are they related to the advantages and disadvantages of the 'servo-mechanism?' We are, from a slightly different point of view, back again to the question of 'control' in machine design. For it is certain that particular types of design will exploit the advantages and disadvantages just referred to more fully than others.

At the same time, interesting theoretical problems are involved. To make any advance, the fundamental types of bodily movement—and of their accompanying perception—must be analysed and experimentally studied, particularly in relation to extent, direction, change of direction, and speed or timing, and to the accuracy of all of these in relation to whatever standard may be required.

Figure 3 shows an experimental arrangement for one study of this type. The operator in this case must try, by a hand and wrist controlled movement, to direct the pointer from the lower line on the drum to the upper, without seeing what he has done. When he has moved the pointer back he can look at the record and try again. Before long he gets the 'feel' of the gear-ratio of the control and settles down to a steady performance. The operator's error in trying to hit the upper line exactly is recorded, and a normal curve of error is shown in the graph (figure 4). It will be seen that relatively small errors occur with control movements of this kind when the amount of movement required lies between 30 and 250 mm. When, however, the extent of movement is reduced below about 10 mm., a large and very rapidly accelerating amount of error must be expected.

In much the same manner, exact knowledge is now rapidly accumulating which shows the effect upon accuracy of direction, path, speed, and

loading of many basic movements carried out by different bodily muscle groups in conjunction with the different kinds of display studied in experiments already mentioned.

Reference to figure 4 reveals another characteristic which is turning out to be true of a wide range of human functions. Variations in the extent of movement, in this case within certain limits, make very little difference to accuracy. Then comes a turning point. A little change of extent beyond this limit means a rapid acceleration of errors. Exactly the same is true of many performances depending on the use of the eyes, the ears, the comparing, discriminating, and perceptual mechanisms, and indeed, of functions at every level of response of which man is capable. To determine limiting boundaries inside which variation of objective conditions is accompanied by very little change of performance, but outside which even minimal change may produce significant improvement or deterioration, is likely to be a more and more frequent task for the experimental psychologist in the near future [6].

III. TESTS

In view of the current wide-spread interest in psychological tests, and of their energetic exploitation, something ought to be said about them here. The aims of a psychological test and of a psychological experiment are quite different. The latter attempts to determine and measure the effects of conditions and changes of conditions upon whatever function is being studied, chiefly with a view to the explanation of that function in terms of its causation. The former is diagnostic and essentially practical. It confronts the subject with some situation which is regarded as typical of many others, more often than not a situation which is itself complex and may be unanalysed. It finds some reliable way of scoring or assessing the subject's performance in this situation, and on the basis of this a prediction is made about his probable behaviour in other situations. The tester often provides problems for the experimenter; the experimenter may suggest methods to the tester, but their aims and procedures remain diverse.

Tests, both reliable and valid, are available for symbolic intelligence whether verbal or numerical. Within far stricter limits than is commonly realized or admitted, they are available also for a variety of special skills. As yet they exist only in an elementary and uncertain form for temperamental and personality characteristics. The greatest needs for test development at present lie along the lines of the improvement of tests of temperament, the extension of tests of special skill, and the devising of tests for whatever it may be that is usually called 'practical intelligence.' For tests in general, the most urgent requirements are improvements of scoring methods, and a very rigorous and critical consideration of the assumptions involved in the statistical treatment of results. Most, if not all, tests are of statistical significance only. They say nothing about the individual directly, but only what proportion of a given class of individuals may be expected to succeed or fail within a given range of operations.

Figure 5 ('jumping monkey') depicts a test situation of the skill variety. The subject holds a lever which is free to move horizontally or vertically, controlling the position of the pointer shown to the right. On a block of squared paper (white in the photograph) to the left is a small steel ball. The subject determines its position, moves the pointer until its tip is exactly over the corresponding position on the squared paper to the right. He presses the lever and if his judgment and movements have been accurate the ball immediately moves to a new and unforeseeable position. By now pressing a morse key, the subject stops the movements of the ball, and repeats the whole operation for as often as may be desired. He works always under pressure. The pattern of squares on the right may be the same as, or in any assigned way different from, that on the left.

Obviously this situation and the performance are both highly complex. The performance involves speed, precision, and delicacy of movement and touch, accuracy of visual location, and capacity to deal swiftly with space co-ordinates. To be successful, the subject must keep calm and collected under pressure. No attempt, however, is made to score these items separately. The score is the time taken for each 'block' of operations. The whole activity involved has the interesting property of being normally subject to highly consistent and rapid improvement. The most significant measure seems to be whether a given individual falls in or outside these normal limits of improvement [7].

The qualities required for success in this case appear to be positively related, in some way not fully understood, to certain aspects of 'practical intelligence.' Demonstration of this is still incomplete, but the test has been included here because it lies along the line of one of the growing developments in this part of current psychology.

Tests can certainly be used with success to select people with high or low levels of intelligence or skill. In use they are economical, and, provided reasonable facilities for follow-up studies are available, they can be validated more exactly than other methods of selection. They are very much less satisfactory with that largest of all class of people and operations where only a moderate range of ability is present or required. Even from a practical point of view, therefore, it is not too much to say that more may be expected from experiments designed to show the character, causation, and distribution of normally functioning human activities, than directly from the use of selection tests. Both are needed, but any step towards a more complete theoretical understanding of the determining conditions of human behaviour in any of its forms advances the time when most practical operations will be designed to bring display, control, and general working conditions all into line with the normal range of limits of widely distributed human capacities. Tests can then be assigned their proper function: the economic identification of extremes, whether of capability or of defect.

NOTE. Most of the work described in that part of the foregoing article which deals with current developments has been or is being pursued at the Psychological Laboratory of the University of Cambridge. Much, though not all, of it is due to the Medical Research Council Unit in Applied Psychology which is established there with Dr K. J. W. Craik as its Director. It would, however, have been possible to take any other active centre of psychological research at the present time and use its work to illustrate the same points.

REFERENCES

[1] MILES, W. R. 'Teaching, Practice and Mental Longevity', *Science*, **81**, (2091, 79–87).

[2] I have given a very brief further account of this work, details of which are still unpublished, in *Proc. Phys. Soc.*, **55**, 417–425. 'Current Problems in Visual Function and Visual Perception.'

[3] Many further details are discussed in my Ferrier Lecture, *Proc. Roy. Soc. B*, **131**, 247–257. 'Fatigue following Highly Skilled Work.'

[4] No detailed account of this work has as yet been published. It is being carried out in Cambridge by Dr D. Russell Davies, who is a member of the M.R.C. Unit in Applied Psychology.

[5] The work referred to is unpublished. It has been carried out by Dr I. M. Frankau, also of the M.R.C. Cambridge Unit.

[6] The experiment used as an illustration in this section is one of a number of closely related investigations due to Dr K. J. W. Craik.

[7] This is one of a number of tests recently developed and used at Cambridge. No detailed report has so far been published.

Note on museums

A recent note on the subject of Museums (ENDEAVOUR, October 1944, Vol. III, p. 134) brings to mind some activities at the National Gallery, London—relating, of course, solely to paintings, but illustrative of the main point in question, i.e. the capacity of museums and galleries to foster research and to further education. One such attempt is now but a memory of desire unfulfilled, but possibly of interest. Plans had been made for an exhibition at the National Gallery in October 1939 of what scientific methods could do for the conservation of pictures, and as a means of probing the technique of the great masters of classical art. Alongside the actual pictures in question were to be shown X-ray, ultra-violet, and infra-red photographs, together with photomicrographs of selected areas.

Since then, on suitable occasions, the exhibition of the 'Pictures of the Month' has been accompanied by X-ray and other special photographs, to bring out points of historical interest, and the public have welcomed such efforts to make looking at great pictures still more entrancing. For the scientist, there is much to be gained by these co-operative ventures, covering between them an expanding field of human endeavour.

IAN RAWLINS

John Harris and his *Lexicon Technicum*

D. McKIE

Dr Johnson, himself compiler of a celebrated dictionary, defined a lexicographer as 'a harmless drudge.' This half-humorous self-depreciation is not altogether justified, for even words *qua* words have a magic. Their precise definition, always desirable, is a prime necessity in science, so that we may pay tribute to John Harris, who secured for Britain the honour of producing the first general technical dictionary in the whole history of science.

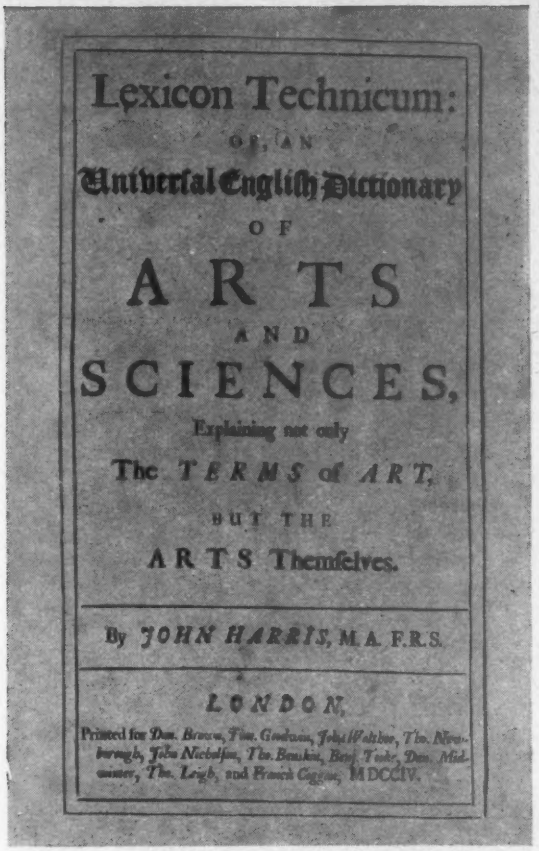
Nowadays technical dictionaries are so familiar to us that we hardly give a thought to their history. We in Great Britain, however, may remember with some feeling of pride that the earliest lexicographer of science was one of our own fellow-countrymen, John Harris, whose *Lexicon Technicum*, published in London in 1704, was the first of its kind in the whole literature of science.

Little is known of Harris's early life, except that he was born about 1666 and that he was a scholar of Trinity College, Oxford, from 1684 to 1688. After graduation, he took holy orders and became vicar of Icklesham, Sussex, and later rector of Winchelsea, and subsequently obtained the Lambeth degree of D.D. He was appointed chaplain to Sir William Cowper, Lord Keeper of the Great Seal, afterwards Lord Cowper and Lord Chancellor of England, through whose patronage he obtained a prebend in the cathedral of Rochester and was presented to the united parishes of St. Mildred, Bread Street, and St. Margaret Moses, London. In right of his prebendal stall he was later appointed perpetual curate of Strood, Kent. In 1715 he became rector of East Barming, Kent.

Harris was much interested in science and was elected a Fellow of the Royal Society on 29th April, 1696. He was chosen to deliver the Boyle lectures for 1698—a series of sermons in memory of Robert Boyle and in proof of the Christian religion against infidelity. About this time, too, he began to lecture on mathematics in the Marine Coffee House in Birchin Lane, London. Mr (later Sir) Charles Cox, M.P., had instituted these free lectures 'for the public good', and Harris delivered them for several years. He also taught mathematics at his house in Amen Corner, 'where any person might be either boarded or taught by the month.' It is of interest to recall that here also, in Amen Corner, fifty years earlier there lived William Johnson, whose *Lexicon Chymicum* (London, 1652–3, 2 vols., and other editions) was the earliest chemical dictionary, though not, of course, a general scientific dictionary.

FIGURE 1 – *John Harris, M.A., F.R.S.* (c.1666–1719), *compiler of the first technical dictionary. Portrait frontispiece to his* Lexicon Technicum (1704), *an engraving by G. White, after a contemporary drawing by R. White, showing Harris at the age of* 37.

Harris's scientific



Lexicon Technicum:

OR, AN

Universal English Dictionary

OF

ARTS

AND

SCIENCES,

Explaining not only

The *TERMS* of *ART*,

BUT THE

ARTS Themselves.

By *JOHN HARRIS*, M.A. F.R.S.

LONDON,

Printed for Dan. Brown, Tim. Goodwin, John Walthoe, Tho. Newborough, John Nicholson, Tho. Bennet, Benj. Tooke, Dan. Midwinter, Tho. Leigh, and Francis Coggan, MDCCIV.

FIGURE 2 – *Title-page of the first edition of the* Lexicon Technicum (*London*, 1704), *the first technical dictionary in the literature of science.*

contemporaries in London seem to have thought well of him. He was a member of the Council of the Royal Society, and became one of the Secretaries in 1709–10, under the presidency of Newton, the other Secretary being Sir Hans Sloane, who succeeded Newton as President. For a short time, it is said, Harris was a Vice-president of the Society.

Harris wrote a number of books on various subjects, some by invitation of the booksellers. These included the printed version of the Boyle lectures for 1698, and sermons in favour of loyalty and against treason and rebellion. Others dealt with astronomy, geography, and the use of the globes; another was a defence of the geological views of Woodward against those of Martin Lister. There were also a sheet of tables for casting up bills of exchange, a collection of voyages and travels, one volume of a projected history of Kent, and a translation from the French of the geometry of Pardies. The most interesting of his scientific writings is the *Lexicon* of 1704. Harris is said to have been very improvident, which probably explains why he wrote for the booksellers; he died on 7th September, 1719, at Norton Court, Kent, 'an absolute pauper,' and was buried in Norton Church at the expense of his friend and benefactor, John Godfrey.

The full title of the *Lexicon* was *Lexicon Technicum: or, An Universal English Dictionary of Arts and Sciences, Explaining not only the Terms of Art, but the Arts themselves.* It first appeared in 1704, in folio, with Harris's portrait as frontispiece. It was dedicated to Prince George of Denmark, consort of Queen Anne. The volume was well illustrated with diagrams and a number of folding plates. It explained over 8,200 scientific terms. In some cases, a word or term was treated in a long article and thus the book is, in part, an encyclopaedia. Harris says in the Preface to this first edition:

> That which I have aimed at, is to make it a Dictionary not only of bare *Words* but *Things*; and that the Reader may not only find here an Explication of the *Technical* Words, or the Terms of Art made use of in all the *Liberal Sciences*, and such as border nearly upon them, but also those *Arts themselves*; and especially *such*, and *such* Parts of them as are most Useful and Advantagious to Mankind.

The *Lexicon*, as was customary at that time, was published by subscription. Among the names of the nine hundred subscribers printed in the preliminary pages are 'Mr Isaac Newton, Master of the Mint,' 'Richard Bently, D.D., Master of Trinity-College in Cambridge,' 'Mr Hawkesbee', and 'Mr Jeremy Clarke, Organist of St Paul's.'

The phrase 'Arts and Sciences' was more widely interpreted in Harris's day than in ours. Thus he deals not only with all that we now recognize as science, as well as navigation, surveying, gunnery, architecture, the rigging of ships, and horology, but also with law, music, art, and heraldry. In the Preface to the first edition Harris states that 'The Catalogue, Right-Ascensions, Declinations, *&c.* of the *Fixed-Stars*, is very imperfect; the Reason of which is, That Mr. *Flamstede*, upon whom I thought I might depend, was pleased to refuse me any Communication of that kind.' Harris showed no sense of grievance over this; for he refers elsewhere to Flamsteed as 'that excellent Astronomer.' It was, however, a time when Flamsteed, owing to his unhappy relations with Newton about the publication of astronomical data, obtained at the Royal Observatory by means of his own instruments, might not be very willing to supply such

information to be included in other men's books.

A supplementary volume to the *Lexicon* appeared in 1710, dedicated to Lord Chancellor Cowper. In it Harris stated that nearly two impressions of the first volume had been sold, and gave a list of more than 1,200 subscribers to the new volume, which contained over 1,900 items, a large number of tables, and the only chemical paper ever published by Newton—the paper on acids, with Latin text and English translation.

Further editions of the *Lexicon* followed during Harris's life-time and others after his death. The book was clearly a success. The fifth and last edition appeared in two folios in 1736, and contained over 12,000 items. In 1744 there appeared a supplementary volume in folio, compiled by 'a Society of Gentlemen,' as the title-page informs us, under the title of *A Supplement to Dr. Harris's Dictionary of Arts and Sciences*. This supplement contained about 4,000 items, so that the fifth edition with its supplement contained about 16,000 items in three large folios.

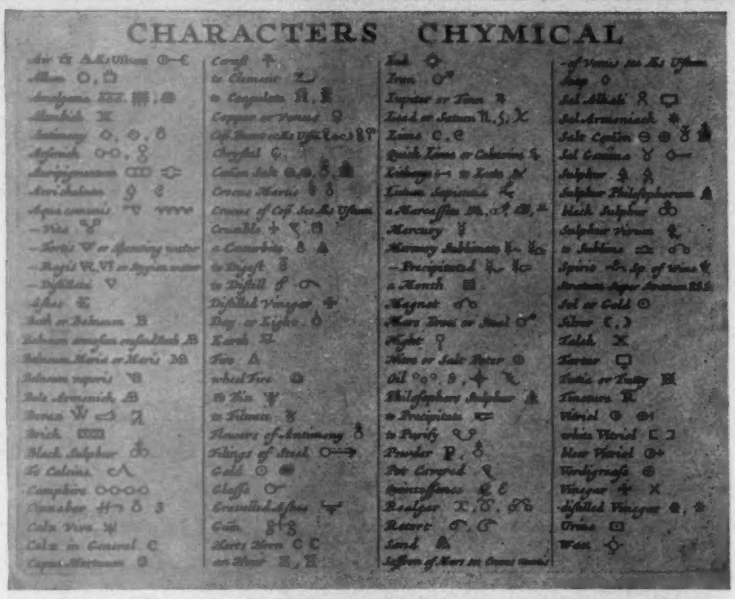


FIGURE 3 – *The table of 'Characters Chymical' or chemical symbols from the* Lexicon *of* 1704. *Probably the finest example of such a table in the older literature of chemistry.*

An interesting way of arriving at an appreciation of the soundness of Harris's work is to consider the terms of one of the sciences in his *Lexicon*. We may very suitably choose chemistry. This was not Harris's special field, for he was a mathematician. Moreover, chemistry, as the eighteenth century opened, was slowly approaching the climax of its long history. The physical synthesis achieved in Newton's immortal *Principia* of 1687 was triumphant; but the chemical synthesis was almost a century away, and chemical thought still retained much that was mediaeval.

The *Lexicon* of 1704 contained about 8,200 items; and about 400 of these were concerned with chemistry, which is a fair proportion in view of the wide range of subjects treated in the book. Under the word *Elements*, Harris states:

> ELEMENTS, is a Word used by Natural Philosophers in the same sense usually as *Principles*; and by the Elements and Principles, or as they sometimes call them, *the Elementary Principles*, of a natural or mix'd Body, they mean those simple Particles out of which the mix'd is composed, and into which 'tis ultimately resolvable.

This was Boyle's definition given in *The Sceptical Chymist* in 1661 as 'certain Primitive and Simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the Ingredients of which all those call'd perfectly mixt Bodies are immediately compounded, and into which they are ultimately resolved.' It was later to be Lavoisier's: and it is still one of the fundamentals of chemical philosophy.

In a longer article under the word *Principle*, Harris gives a good account of the views of Boyle, and writes:

> And in Chymistry particularly, 'tis taken for first Constituent and Component Particles of all Bodies, out of which they are made, and into which they are by Fire, as they say, resolvable again. Thus *Salt*, *Sulphur*, and *Mercury* are the three Famous Chymical Principles, which they call *Hypostatical*; and the Chymists did formerly pretend, that they could by their Art resolve all Natural Bodies into these; and that these Principles could be drawn Simple, Pure, and Uncompounded from Metals, *&c.* But since this Art hath been more commonly studied and consequently much better known, it is found to be a Falsity as Mr. *Boyle* excellently shews in his *Sceptical Chymist*; and *Lemery* hints in many places of his good *Course of Chymistry*.

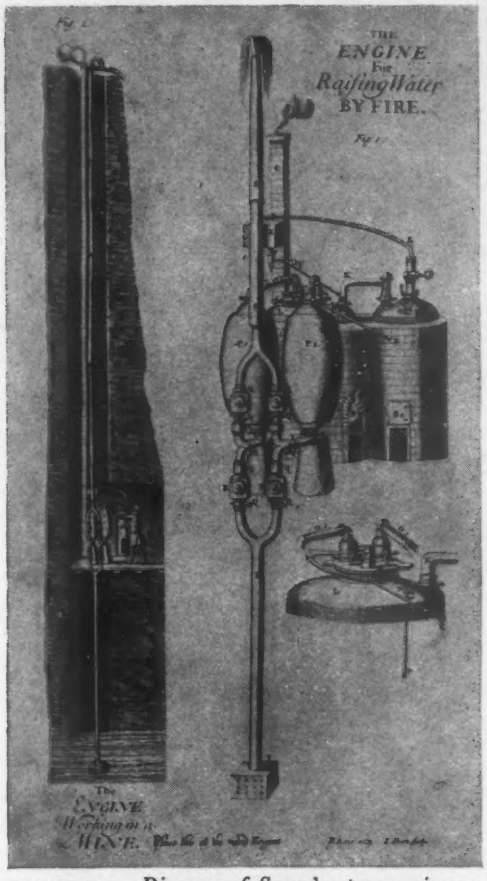


FIGURE 4 – *Diagram of Savery's steam-engine or 'The Engine for Raising Water by Fire' from the* Lexicon *of* 1704. *This engine was first described in Savery's* Miner's Friend (1702).

FIGURE 5 – *Marshall's microscope from Harris's* Lexicon (1704) *with 'a small Fish lying on the Glass-plate, that the Circulation of the Blood may be seen in part of the Tail-fin, at* (c).'

Boyle and Lemery were Harris's chief authorities in chemistry: they were also the best at that time. Three English versions of Lemery's *Cours de Chymie* had already appeared; and Harris took his table of 'Characters Chymical,' or chemical symbols, from Lemery.

Harris's modern outlook is well evidenced in what he says of alchemy. Under the word *Alchymist*, he writes:

> ALCHYMIST, is one that studies *Alchymy*; that is, that Sublimer Part of *Chymistry* which teaches the Transmutation of Metals and the Philosopher's Stone; according to the Cant of the *Adeptists*, who amuse the Ignorant and Unthinking with hard Words and Non-sense: For were it not for the *Arabick* Particle *Al*, which they will needs have to be of wonderful vertue here, the word would signifie no more than *Chymistry*. . . . This Study of *Alchymy* hath been rightly defined to be, *Ars sine Arte, cujus principium est mentire, medium laborare, & finis mendicare:* That is, an Art without an Art, which begins with Lying, is continued with Toil and Labour, and at last ends in Beggery. And so poor *Penotus* found it, who after he had spent his whole Life and Fortune in this vain Study, died at last in an Alms-House at *Yverdon* in *Switzerland*; and used to say, he would recommend the Study of *Alchymy* to a mortal Enemy, whom he did not dare openly to attack.

In another place, he says:

> ADEPTISTS, or *Adepts*, are such Alchymists as pretend to have gained the Secret of the *Transmutation of Metals*, or to make the *Philosopher's Stone:* Of these Mystical, Invisible Gentlemen (they say) there are 12 always in being; which are kept

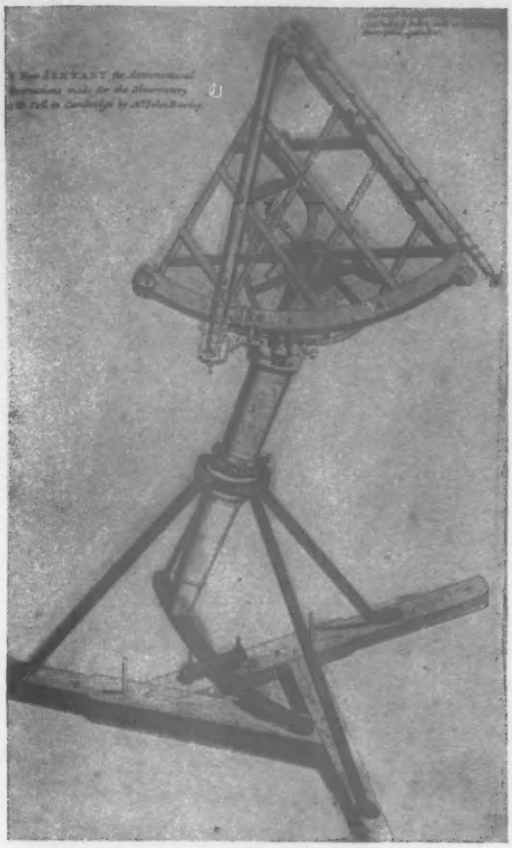


FIGURE 6 – *A sextant by Rowley for the observatory at Trinity College, Cambridge, shown in the supplementary volume* (1710) *of Harris's* Lexicon.

supplied by new ones when any of the Fraternity pleaseth to die, or to translate himself to some place where he can make use of his Gold, for in this wicked World it will not procure them Shirts.

We may perhaps regret the passing from chemistry of such picturesque terms as 'Salamander's Blood' for the red fumes that fill the receiver towards the end of the distillation of 'Spirit of Nitre,' but Harris has no mercy on them: it is 'a foolish Term.' So also 'Alkahest' is 'one of the Canting Terms of the Alchymists.' As for the so-called 'powder of sympathy,' Harris writes:

SYMPATHETICK *Powder*, is only (said *Lemery*) Green, or *Roman Vitriol*, opened by the Sun Beams penetrating into it, and imperfectly calcining it: The Vitriol is usually exposed to the Sun's heat in the Month of *July*, but some use only Powder of Vitriol. When they use it, they spread some of the Powder upon a Linen-cloth dipt in the Blood of any Wound, and then pretend, that if the Cloth be many Miles off the wounded Person, yet he shall be healed: But this is so far from being true, notwithstanding the vaunting Stories of Sir *Ken. Digby* and others, That 'twill hardly have its Effect if done in the same Room where the Patient lies; and he certainly is very much wanting to himself, who will use no other help.

Vitriol hath its Parts in continual Motion; and 'tis probable, many *Effluvia*, may go out from it; and some few of those, if the Application be made to the Cloth, just by the Patient, may perhaps enter into the Wound, and help stop the Bleeding, for Vitriol is a known Styptick; but he that will neglect all other Means, and depend only on this, may probably pay dear for his Credulity.

With regard to heat (always as much a part of chemistry as of physics), Harris says that it is 'one of the four Primary Qualities' and that it seems to consist only, or at least chiefly, 'in the local Motion of the small Parts of a Body Mechanically modified by certain Conditions, of which the Principal is the vehement and various Agitations of those small Insensible Parts,' and he emphasizes that the particles of the body must be agitated, not only 'vehemently and rapidly,' but also in such a way that their 'Determinations,' i.e., the directions of their motions, must be '*very various;* some moving up, some down, some to the Right Hand, others to the Left, *&c.*' That heat consisted of material particles was also believed in Harris's day, and almost into our own time, but he does not refer to this opinion, and it seems that he himself had decided in favour of a dynamical theory.

Harris had, as we have seen, taken Boyle and Lemery for his guides in chemistry; and he had also accepted the atomistic views expressed by Newton in the 'Queries' to his *Opticks*, some account of which has already been given in this journal (1942, Vol. I, pp. 141–4). In general we may say that a reader who consulted the *Lexicon* for some chemical information would find that his needs were well satisfied. Harris's outlook is abreast of that of his time and is directed towards the future.

Much of his excellent work passed into the encyclopaedias and notably into that of Chambers. Before the century was ended, the several sciences began to need their own special dictionaries instead of a universal lexicon. Today it is perhaps worth recalling the pioneer work in this field of John Harris—improvident clergyman, able mathematician, hack-writer for the booksellers and first lexicographer of science.

Clouds and cloudscapes

C. J. P. CAVE

Those who live in a rainy climate are prone to use the metaphor 'a cloudless sky' as meaning complete serenity of prospect. Yet, in common with the rest of nature, clouds possess a beauty and an interest of their own. In a series of remarkable original photographs, most of which are here published for the first time, Mr Cave displays the chief types of cloud. Pictures and text together will make many of us cloud-lovers: *cras amet qui nunquam amavit.*

Very few take heed of the clouds; it must be some very remarkable cloudscape to call for much attention. Yet to those who care to observe them they are not only fascinating, but give indications of coming weather—useful when weather forecasts are banned or not available.

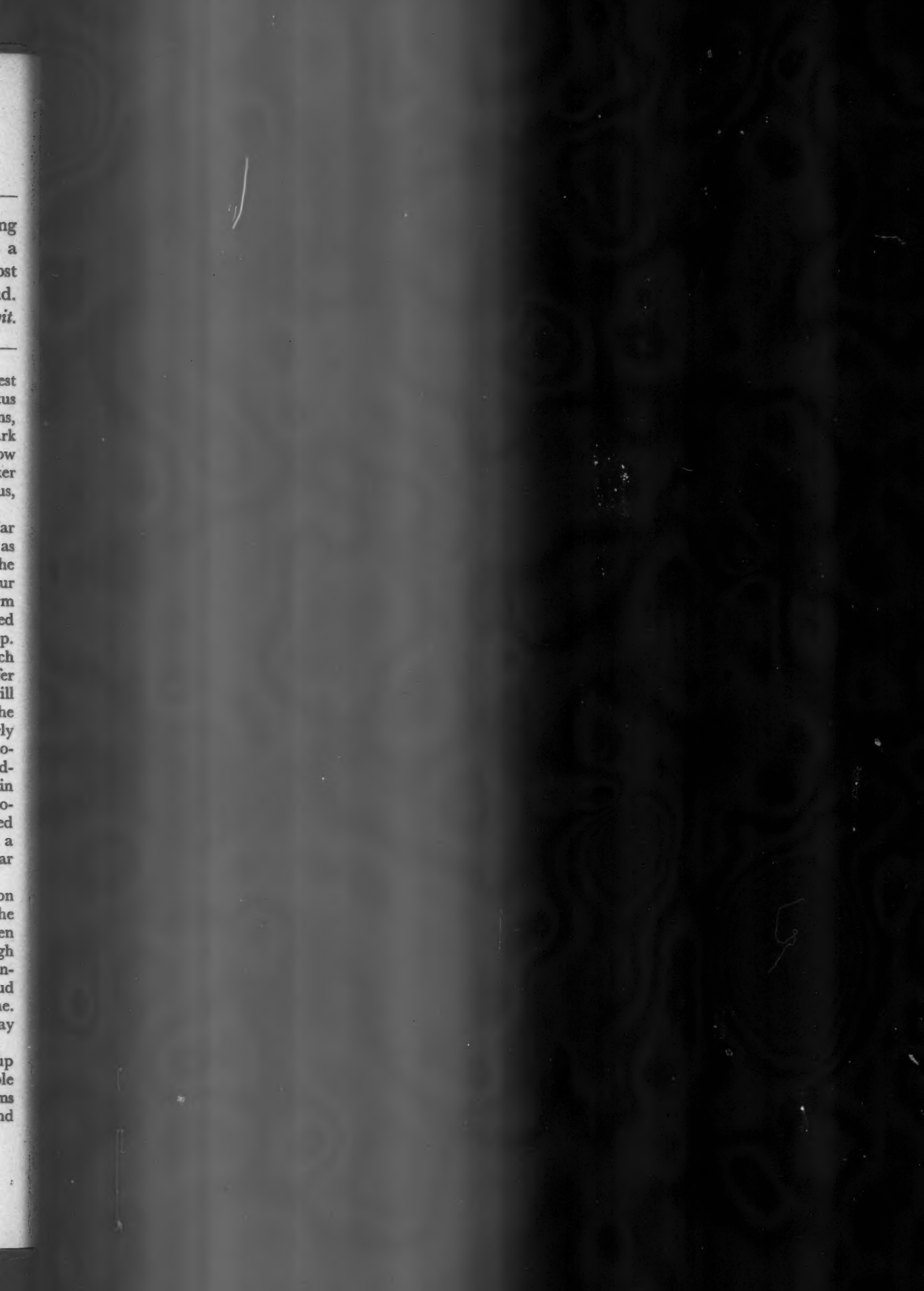
Clouds may be divided into those which occur in horizontal sheets, sometimes continuous, sometimes broken up, and clouds with more vertical thickness, ranging from the small cumulus clouds of a summer day to masses whose heights vie with those of the highest mountains. Clouds are composed either of minute drops of water or of minute ice crystals.

In a short survey we may begin with the horizontal sheets, and deal first with the highest levels, the cirrus clouds, which are always composed of ice crystals. These are very varied in form (figure 1), but they may always be distinguished from the lower clouds by being composed of fine threads and wisps. They are at heights of 20,000 to 30,000 feet. The drawn-out tangle of strands which these clouds often exhibit gives the impression that they are being blown about by the wind; yet it is not by any means always so, for some of the best displays of cirrus occur in fine still weather when there may be hardly any wind between the ground and the stratosphere. If such clouds are seen moving very rapidly from the west or southwest it may well be that bad weather is approaching. When cirrus cloud is spread in a continuous sheet over the sky it is called cirrostratus; the sun may be seen through such a sheet, and there may be haloes or mock suns, but not always. At a somewhat lower level is another cloud sheet, altostratus (figure 2). The difference between the two is not always easy to make out; but altostratus is thicker and darker; it shows no cirrus threads; the sun may shine through it with a watery look, or it may be hidden altogether; haloes are never seen; it is composed of water drops, not of ice crystals. A sheet cloud moving up from the west may be cirrostratus along its front and altostratus further back. Note how the cloud sheet thickens, how the sun becomes blotted out, and how dark detached masses of cloud move below the now continuous sheet (figure 2), which may get thicker and darker till it becomes the formless nimbus, the thick overcast sky of the rainy day or night.

All these clouds make dull skyscapes. Far different are those at comparable levels known as cirrocumulus and altocumulus. These are the clouds that give us our finest skyscapes and our most spectacular sunsets and sunrises. They form sheets broken up into waves, ripples, or detached cloudlets that have been likened to flocks of sheep. They are rather difficult to distinguish from each other; they merge one into the other and differ really only in height. True cirrocumulus will always show some cirrus threads among the ripples or cloudlets, and the latter are pearly white with no shadows (figures 3 and 4). Altocumulus shows no cirrus threads among its cloudlets and waves, and these usually have a certain amount of shadow (figures 5 and 6). Both cirrocumulus and altocumulus are sometimes arranged in long bands (figure 7) which often move from a northerly point, and hence are known as polar bands.

At a lower level is stratocumulus, a poor relation of altocumulus. It has none of the grandeur of the higher clouds, though like them it may be broken up into waves with some blue sky showing through the interspaces. In quiet weather in autumn or winter it may form a nearly continuous sheet of cloud (figure 8) which may persist for days at a time. Stratocumulus, especially on a summer day, may break up later with the formation of cumulus.

Cumulus, as its name implies, is heaped up with more or less vertical thickness; it is the visible effect of rising currents in the atmosphere. It forms on very many fine summer days (figure 9) and



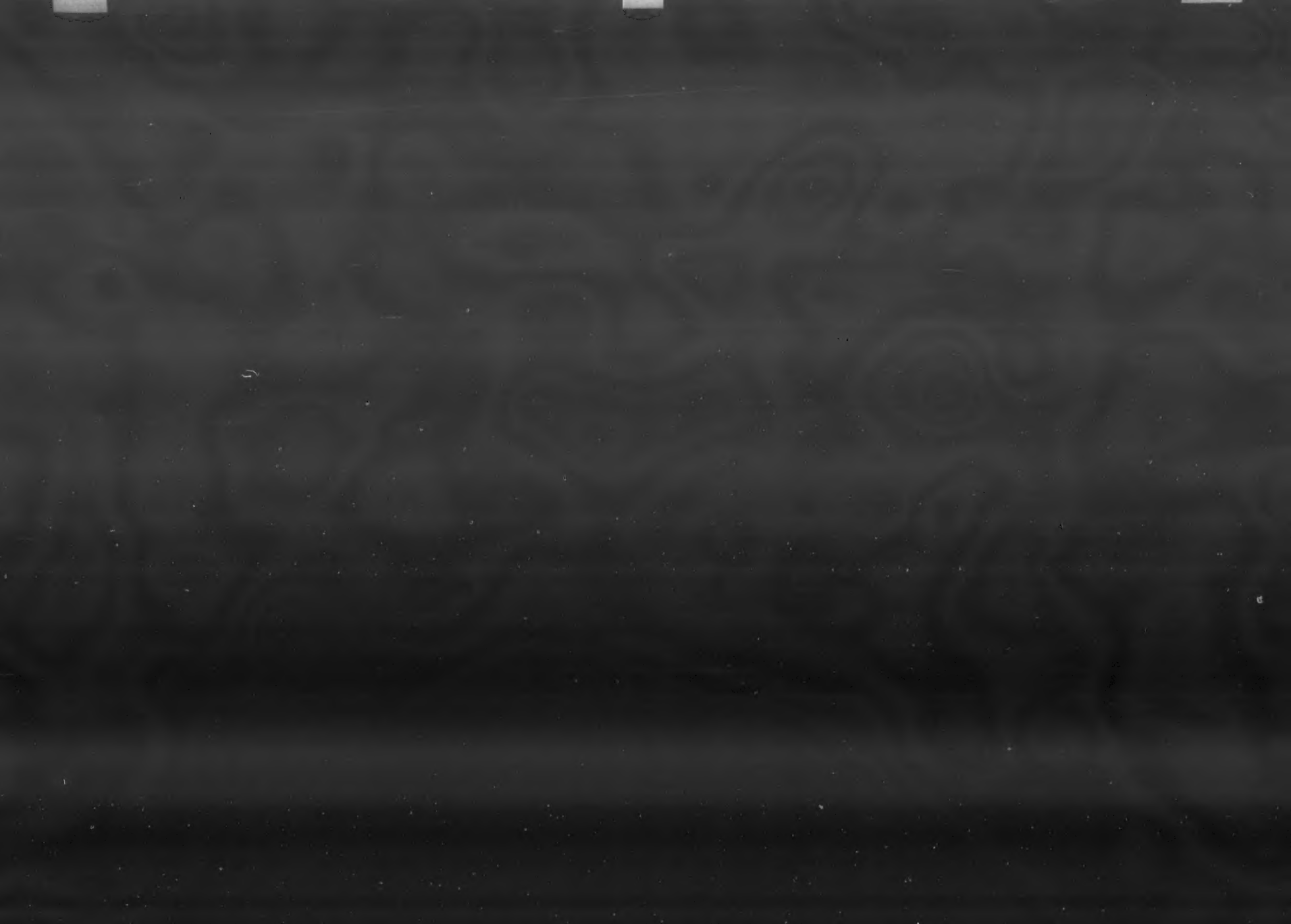


FIGURE 1 – *Cirrus.*

FIGURE 2 – *A sheet of altostratus with dark clouds at a lower level.*

FIGURE 3 – *Cirrocumulus ripples.*

FIGURE 4 – *Cirrocumulus waves and ripples.*

FIGURE 5 – *Altocumulus waves.*

FIGURE 6 – *Altocumulus cloudlets.*

FIGURE 9 – *Small cumulus of a summer day.*

FIGURE 10 – *Cumulus of a fine summer afternoon.*

FIGURE 11 – *Towering and growing cumulus, the kind of cloud likely to develop into cumulonimbus.*

FIGURE 12 – *Cumulonimbus, a distant shower cloud; note the soft edged summits—the hybrid cirrus.*

FIGURE 13 – *Cumulonimbus; an approaching thunderstorm; note the anvil-shaped top.*

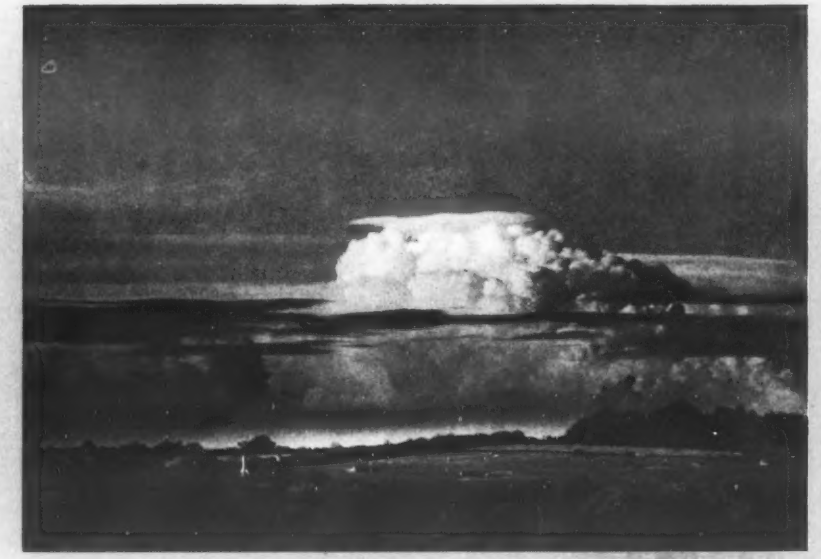


FIGURE 14 – *Stratus showing dark bands in front of a distant thunder cloud.*

after growing larger in the afternoon (figure 10), decreases later and disappears in the evening; but under suitable conditions it may grow to towering heights (figure 11). On a day when cumulus clouds are showing signs of rapid growth, if we watch the top of a large cloud we may see a change come over it. The top from being rounded and hard-edged becomes soft and fibrous (figure 12); the water-drops in the upper part of the cloud have frozen into minute ice crystals which are driven upward and outward by the up-currents to which the cloud itself is due. When the change takes place the cloud is known as cumulonimbus; it has become a shower cloud and perhaps a thunder cloud. An intense thunder cloud may have the whole of its top converted into a mass of hybrid cirrus, as it is called, and this may extend out sideways and give the upper part of the cloud the shape of an anvil (figure 13). On a hot summer afternoon when the weather has begun to be thundery one may see these anvils on the horizon and know that each one is the site of a shower and very likely of a thunderstorm.

On a normal thundery day in summer the clouds lose their lift in the latter part of the afternoon, the showers cease, the last peal of thunder dies away, and by nightfall the clouds have dissolved. But, especially in the late summer, conditions may favour the development of thunderstorms during the night, when very extensive masses of cumulonimbus clouds develop, giving rise to widespread storms and displays of lightning.

These are the main forms of clouds. There are many varieties of lesser importance, but some are of much interest. Stratus clouds have been likened to a fog not in contact with the ground. These clouds occur in detached masses at varying heights; they taper off at the edges and are thicker in the middle. They tend to develop among mountains, and also in the neighbourhood of large thunder clouds, where they form dark lines across the gleaming white of their background (figure 14).

Lenticular clouds are rather like high stratus; they are thickest in the middle and thin out towards their edges, which are soft, constantly changing, and often rippled. These clouds are sometimes lens-shaped, and then, viewed in profile, they are like a convex lens seen edgeways. They are at a high level, probably generally between 15,000 and 20,000 feet. If they are watched carefully it will be seen that they are dissolving on the leeward side and forming on the windward side, so that though a strong wind may be blowing at this level, and though the cloud particles are moving with the wind, the cloud itself remains stationary (figure 15).

Another cloud to be noted is turret cloud, or altocumulus castellatus. Here the cloudlets of altocumulus are drawn up into what look like miniature heads of hard-edged cumulus, and sometimes from a flat base a series of cumulus-like heads rises up (figure 16). These clouds produce very beautiful effects, and they are important because in summer they are frequently the precursor of thundery weather.

Condensation trails are artificial clouds formed, usually, by hot moist gases from the exhaust of an aeroplane mixing with very cold moist air in the atmosphere. When they form, it is usually at the cirrus level where some natural cloud is already forming. After some time a condensation trail may spread out over a wide area and become indistinguishable from ordinary cirrus, or cirrostratus; the former, however, seems more prone to cause mock suns and haloes than cirrostratus formed naturally.

Rheology: the dynamics of real fluids

E. G. RICHARDSON

'All is flux,' said Heracleitus, and the world had to be content with the aphorism for 2,200 years. Then the motion of fluids was studied by Newton, on whose work our fundamental conceptions of the subject are still based. In recent years, and particularly under the stress of war-time requirements, much research has been carried out on the dynamics of fluids, and in this article Dr Richardson describes the new science of rheology.

It is a matter of common knowledge that our fundamental ideas on the subject of the motion of fluids, and of the resistance experienced by bodies exposed to a current of liquid or gas, came from Newton. It was Newton who envisaged the flow over a solid boundary as being characterized by a gradient of velocity, the fluid in contact with the boundary being at rest, while successive 'layers' of fluid (going outwards from the boundary) move with greater and greater speeds until the speed of the free stream is attained. The fluid is thereby sheared in the same way that a slab of jelly is sheared if, while its lower surface adheres to a plate, the upper surface is pushed aside by the hand. The rate of shearing is proportional to this gradient of velocity parallel to the plate, but the motion also depends on the physical properties of the fluid.

Much later, this conception was taken up by Poiseuille, who used it to compare the behaviour of liquids when subjected to shearing forces during flow. It is to him and to Hagen that we owe the definition of 'coefficient of viscosity' as the property of a fluid which relates the shearing force to the gradient of velocity at the solid boundary which sets up the shear. Poiseuille sheared the liquid by making it pass through a narrow tube and—with the aid of Newton's law of flow—relating the mean rate of flow through the tube to the dimensions of the tube and the viscosity of the liquid. In order to overcome the resistance of the tube to the passage of the liquid through it, and thereby to shear the liquid, pressure must be exerted to force the liquid through the tube, and energy is dispensed in the fall of 'head' between the liquid levels at the two reservoirs connected respectively one to each end of the tube.

Although Poiseuille did not attempt to measure how the velocity of the fluid fell from its maximum value at the axis of the tube to zero at the walls, he established theoretically that it must fall in parabolic fashion, i.e. that, passing from the wall to the centre, the velocity should increase as the square of the distance from the wall. The distribution of velocity across the tube is represented in figure 1(*a*) by the respective lengths of the horizontal arrows.

FIGURE 1

Even solids may show this Poiseuille type of flow under certain conditions. Figure 2 is a photograph of a section of a billet of metal extruded under high pressure through a die. The billet was originally cylindrical but was cut longitudinally into halves before extrusion and horizontal stripes were painted on the exposed flat surfaces. After putting the halves together again, extruding, and re-separating, the stripes are seen to be distorted by flow through the orifice into a pattern closely resembling the parabola of figure 1(*a*).

In a liquid which obeys Newton's law, increase of pressure or rate of shear on the liquid will increase the velocity gradient and the rate at which the liquid passes through the tube *in the same proportion*. In other words the viscosity is a constant factor, unique for every 'Newtonian' fluid, though it varies widely from one fluid to another. A number of liquids and gases which are homogeneous in the sense that we cannot by any means at our disposal detect that they have different properties in different parts, or when viewed in different directions, do obey the simple laws of viscosity; but in recent years increasingly numerous fluids have been found to have different coefficients of viscosity at different rates of shear. On examination, it proves that fluids with this

abnormal behaviour, 'non-Newtonian fluids,' are heterogeneous; for example, they may have particles of a different nature floating about in the body of the liquid (*sols*), or possess a structure binding the mass together in a semi-solid *gel*.

It is interesting to reflect that Poiseuille himself might have stumbled on this fact. He was a physician interested in the mechanism by which the blood passes into the smaller blood vessels or capillaries, and did his experiments with blood solutions forced through narrow tubes. Now blood is not a homogeneous liquid; it has corpuscles floating in it, but not in sufficient concentration to show much change of viscosity with rate of shear.

Sometimes the existence of ambient particles or of a structure can be detected by visual inspection with the unaided eye, but often the particles are too small to be seen without a powerful microscope, though the fact that the fluid is heterogeneous may be established by their ability to scatter light. This was first demonstrated by

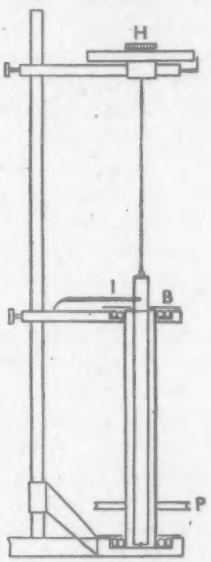


FIGURE 4

Tyndall. If a conical beam of light is passed through pure water, its passage is invisible against a dark background, but if the same beam is passed through a *sol* or a *gel*, its track through the liquid becomes visible by the light scattered (figure 3). Such are non-Newtonian fluids.

Figure 1(*b*) shows how the velocity of flow of a markedly non-Newtonian liquid varies across a tube in which it is flowing. The velocity is nearly constant across the major portion of the tube and falls in a steep gradient to the walls. This type is accordingly known as plug flow. Although it is very difficult to measure the velocity from point to point across a narrow tube, it may be done if the liquid is contained in the space between two coaxial cylinders, one of which—usually the

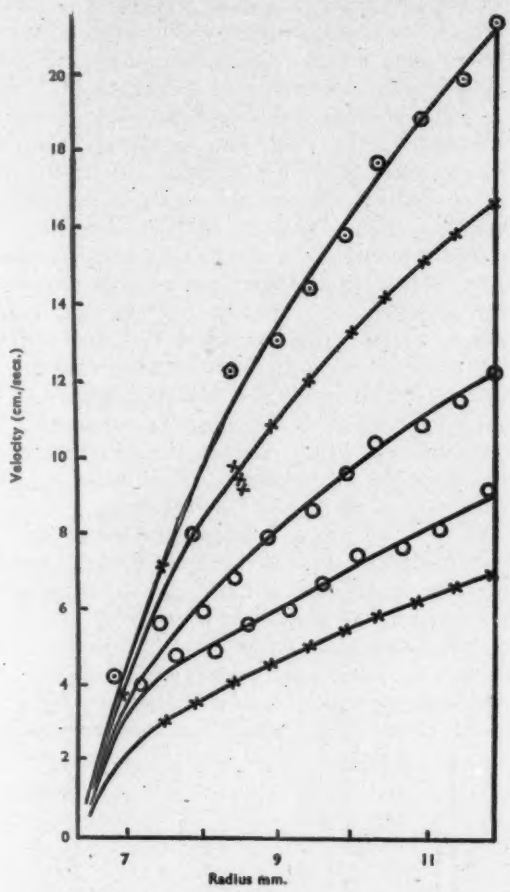


FIGURE 5

outer—is rotated while the other is held at rest. The liquid moves in a series of concentric rings and is sheared in the process. Figure 4 shows an appropriate apparatus in section [1].

The outer cylinder is held vertically in bearings B and may be rotated by an electric motor working through a belt and the pulley P. The inner one is held by a wire from a torsion head H and its deflection under the shear transmitted through the liquid is recorded by the motion of the pointer I moving over an horizontal dial. In a Newtonian liquid, the velocity of the liquid at any radial distance from the inner cylinder is

proportional to the distance in question, so that in figure 5, where the local velocity is plotted against the distance between the cylinders, a line joining the plotted points would be straight from the known velocity of rotation of the outer cylinder to zero at the inner. But in the clay paste, to which the graph applies, it is apparent that the velocity falls more steeply in a curved line at the inner cylinder. (The various curves relate to different rates of rotation of the outer cylinder; for the method of measuring the point-to-point velocities, the original paper must be consulted [2].) It will also be noticed that the more slowly the outer cylinder is rotating the more constant the general velocity over the inter-space becomes, with a sharper drop to the stationary surface of the inner one. At high speeds of rotation of the outer cylinder, however, the velocity falls steadily towards the inner, almost as when the apparatus contains a Newtonian liquid. The twist on the inner cylinder is, in fact, a measure of the velocity-gradient at its surface, and, even if we cannot measure the velocity-gradient directly, we can deduce it from the twist on this cylinder as indicated by the deflection of the pointer I (figure 4).

If then we put a homogeneous liquid in the apparatus, the ratio of this twist to the speed of rotation of the outer cylinder is a constant, proportional to the coefficient of viscosity; but with a non-Newtonian liquid the torque increases rapidly at first (as the outer cylinder speeds up) and then more slowly, so that the ratio: torque/speed of rotation (called the *apparent viscosity*) decreases as the rate of shear on the liquid increases. This is a common feature of these anomalous liquids and one of considerable technical importance. Figure 6 shows how the viscosity of a clay suspension decreases with increasing velocity-gradient, whereas that of the solute (water) remains constant.

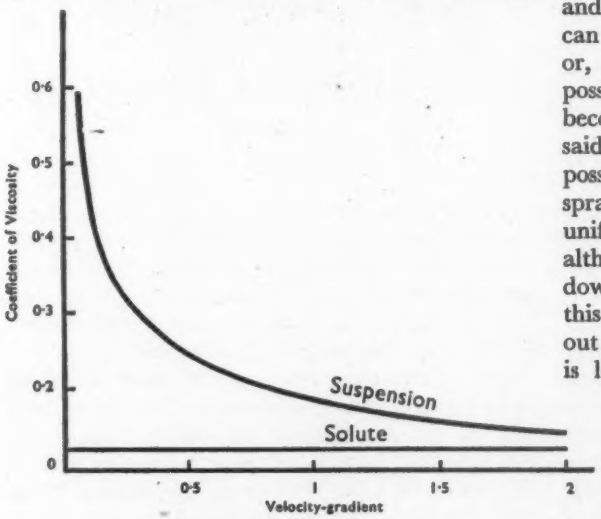
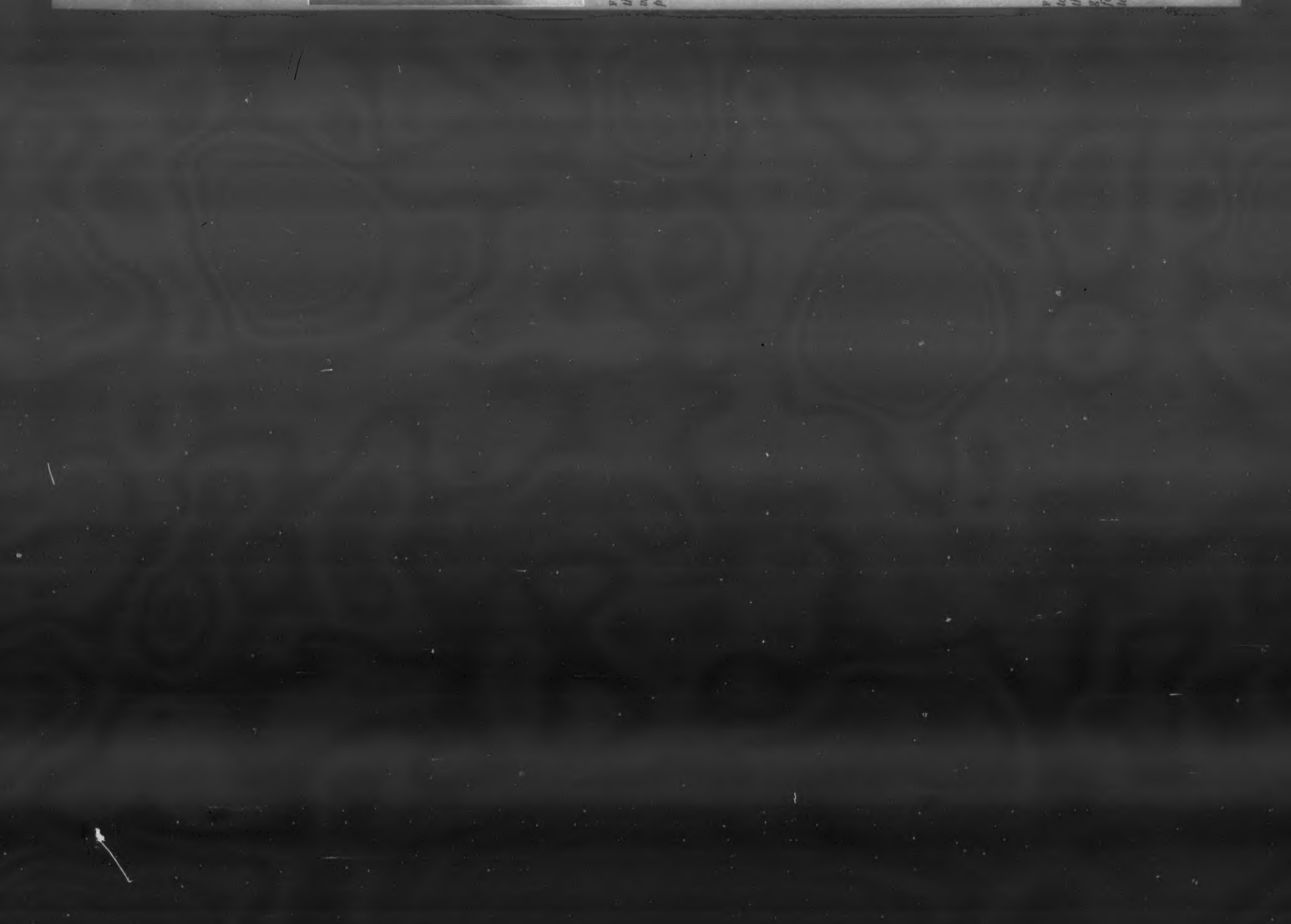


FIGURE 6

In accordance with Newton's law, a homogeneous liquid has also this property, namely, that it begins to move under the smallest possible shear. The smallest appreciable fall in pressure along a tube, or the smallest possible application of turning moment to the outer cylinder in the apparatus of figure 4 is enough to start the liquid moving. When, however, the concentration of the material in suspension in the liquid is high—when it forms, in fact, a fairly stiff paste—it is found that the substance will bear small shearing forces without starting to move. The existence of such a 'yield value' in, for example, pastes of modelling clay makes it possible to mould them into a shape which they will retain for some time—they are in fact, plastic. In a rather similar fashion, confectionery jellies may be set in a mould on a plate and after removal of the mould the mass will retain its shape, in spite of the force of gravity urging it to flow out over the plate.

The setting of a gelatine solution is an interesting case in which a non-Newtonian liquid—the *sol*—acquires internal structure by the reduction of its temperature and becomes a *gel*, possessing both yield value and elasticity. The latter factor is evidenced by the way in which jelly, partly displaced from equilibrium, 'shivers' to and fro and eventually returns to equilibrium. The *gel* can be transformed into the *sol* again by heating, or, sometimes, by shaking. A system which possesses *gel* characteristics when left alone, but becomes—temporarily—a *sol* when disturbed, is said to be thixotropic. A good paint should possess this property, so that it can be brushed or sprayed over a surface smoothly and retain a uniform texture and gloss when left. Further, although it has a high viscosity when settling down slowly over the surface it has to cover (and this promotes a good 'finish'), yet when forced out of the spray nozzle at high speed its viscosity is low and energy losses in the nozzle are so reduced to a minimum.

Finally, some liquids, particularly solutions of rubber in toluene, exhibit bulk elasticity. If a rubber solution is forced through a capillary tube, it is found that on emergence the liquid swells out to a



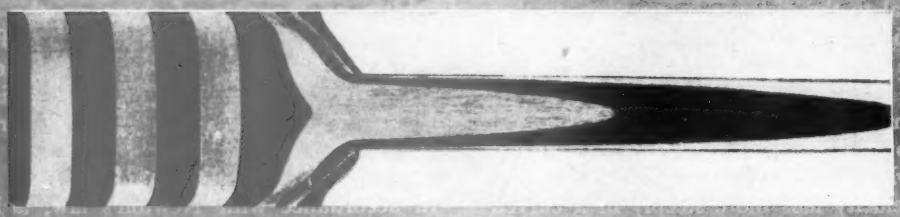


FIGURE 2 – *Photograph of a section of a billet of metal extruded under high pressure through a die. Originally cylindrical, the billet was cut longitudinally into halves before extrusion, and horizontal stripes were painted on the exposed flat surfaces. After putting the halves together again, extruding, and re-separating, the stripes are seen to be distorted into a pattern closely resembling the parabola of figure* 1 (*a*).

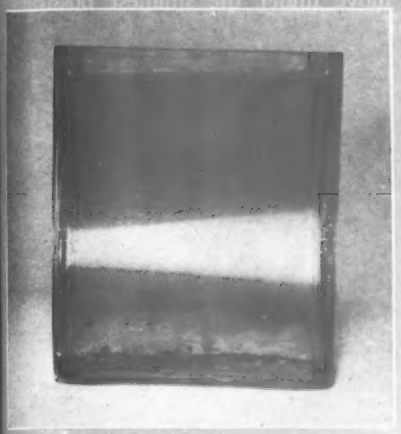


FIGURE 3 – *Track of a beam of light through a heterogeneous fluid. It is rendered visible by the scattering caused by the minute particles suspended in the liquid.*

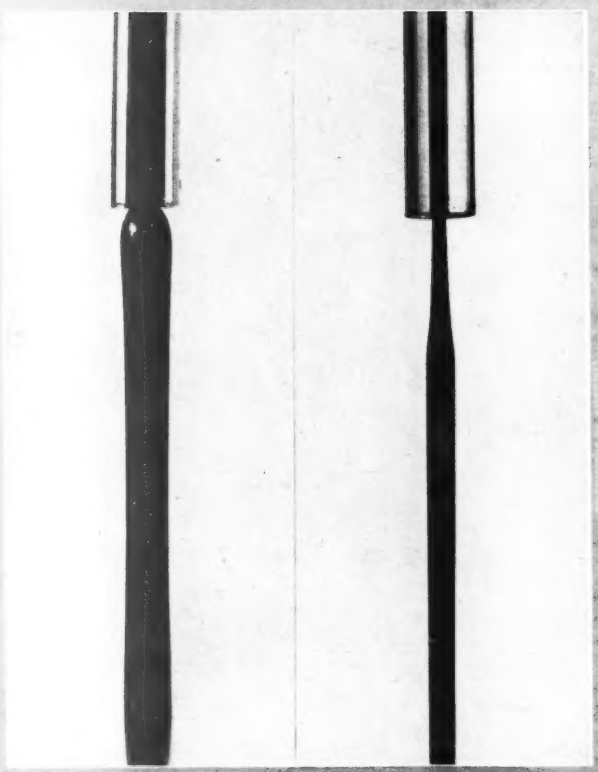


FIGURE 7 – *Bulk elasticity. A solution of rubber in toluene is forced through a capillary tube. It is found that, on emergence, the liquid swells out to a diameter greater than that of the tube. The pressure used to force it through the tube must therefore have caused it to contract on entry.*

diameter greater than that of the tube [3] (figure 7). Evidently, under the influence of the pressure used to force it through the tube, it had contracted on entry and relieved itself of this compression on leaving the tube.

The importance of this property can be realized when it is pointed out that, besides rubber solutions, such diverse materials as dough, cheese, cream, and animal muscle have been found to exhibit elastic after-effects when forced to flow.

The elasticity exhibited by such liquids links them in behaviour with solids, for it is primarily to a solid that we ascribe a proportional strain resulting from an applied stress (Hooke's law). In a true solid, however, this relationship is independent of time, while an inviscid fluid is incapable of sustaining a strain unless it is contained in an hermetically sealed vessel. But if a liquid possesses viscosity, has room to flow, and is stressed, the stress will dissipate itself (by the movement of the fluid) at a rate which Clerk Maxwell assumed to be proportional to the strain existing at any instant. Thus, on the application of the stress, the comparatively large initial strain results in a rapid flow, which decreases as the strain is relieved. The factor of proportionality is related to the time which the liquid takes to relax, namely, to relieve itself of the stress applied to it. Maxwell showed that this 'relaxation time' should be equal to the quotient of the coefficient of viscosity by the modulus of rigidity.

The relaxation time can be measured by means of the apparatus shown in figure 4. If, for example, a solution of rubber in toluene is placed in the space between the cylinders and the outer cylinder is rotated through 90° and then held still, the inner one will follow quickly through a smaller angle, indicating that the *sol* is able to bear the torque (measured by the difference in angular position of the two cylinders) at first. It will not hold this stress indefinitely, and after an initial rapid deflection of, say, 60°, the inner cylinder will gradually creep up to the same deflection (90°) as was given to the outer one. If a true liquid is put in, the inner cylinder *at once* copies the deflection of the outer one. (It is assumed in these experiments that the thread supporting the inner cylinder is long and incapable of exercising appreciable restraint on its movements.) Ability to stand a small permanent deformation is shown by certain *sols* which leave the inner cylinder permanently at a smaller deflection than the outer one is given.

We have written so far of those systems in which a solid is 'dissolved' either as a true solution (like 'rubber solution') or as a suspension (like clay in water). There is another class of non-Newtonian fluids in which both phases are fluid. These are the *emulsions*, in which one liquid subsists as globules in another, forming the continuous phase; and the *foams*, in which a gas is dispersed as bubbles in a liquid. Both require the presence of emulsifying agents (of which soap is a common example) if they are to remain stable and not to settle out into two discrete constituents, one above the other. The action of such an agent is rather obscure, but it is believed to be connected with the electrical sheaths with which it is able to surround the drops or bubbles. Whatever the consistency of such *sols*, whether definitely fluid like mayonnaise and bath foam or solid like margarine and sponge chocolate, these systems are of considerable technical importance. They all exhibit in the concentric cylinder apparatus a viscosity decreasing as the rate of shear increases. With the stiffer emulsions there is a critical velocity—something analogous to the yield value already referred to—below which the substance does not flow at all. Below this speed the emulsion behaves as a solid, above it as a fluid with rapidly decreasing viscosity as the 'structure' within it is broken down under stress [4].

Much has been learned about the behaviour of non-Newtonian liquids during the course of war research; much still remains to be learned. It is now evident that many of the liquids used in everyday life in both domestic and industrial spheres are of this type; hence the importance of a continuing study of their flow-properties [5] comprised in the new science of 'rheology.'

REFERENCES

[1] RICHARDSON. *J. Agric. Science*, 1933, **23**, 176.

[2] RICHARDSON and TYLER. *Phys. Soc. Proc.* 1933, **44**, 142.

[3] MERRINGTON. *Nature*, 1943, **152**, 663.

[4] RICHARDSON. *Koll. Zeits.*, 1933, **65**, 32.

[5] For a general survey of the technical aspects of rheology the reader is referred to a recent text-book by Scott-Blair, *Introduction to Industrial Rheology*.

Agar-agar and its supply

LILY NEWTON

The most important bacteriological medium is agar-agar, a gelatinous product obtained from certain seaweeds. Its pre-war production was virtually a Japanese monopoly, and the outbreak of war therefore confronted the Allies with the problem of obtaining this vital commodity in adequate amount. Many countries met the need by the utilization of local seaweeds. In Britain a satisfactory agar has been produced, the botanical work being under the direction of Professor Lily Newton, who here gives a world survey of the agar-agar position.

Agar-agar is a commercial product which is made from various seaweeds. Up to 1939, its manufacture was largely a Japanese monopoly, though other countries produced a certain amount. The U.S.A., though a producer, was also an importer. Small amounts were made in Russia, Australia, New Zealand, South Africa, and on the coasts of the Indian Ocean, especially from Ceylon to Burma and Malay. The outbreak of hostilities materially changed the world position, and many attempts have been made to increase the supply.

The agar-producing weeds belong to the *Rhodophyceae*, which are red in colour, the shade varying from bright rose-pink in deep water to deep brownish-red in places more exposed to the light. Their plastids contain, in addition to chlorophyll, phycoerythrin and sometimes phycocyanin and fucoxanthin. The product of assimilation is a solid polysaccharide, similar to starch and known as Floridean starch. The middle lamella of the cell-walls is swollen and impregnated with a mucilaginous substance which, in many cases, gives pentoses or related substances on hydrolysis. Sauvageau called the mucilaginous substance gelose. It constitutes the raw material of agar-agar and other gelatinous substances obtained commercially from the seaweeds.

Algae vary in their normal length of life, but the terms annual, biennial, and perennial, as applied to land plants, have not quite the same significance when applied to seaweeds. In the latter, in the case of annuals, new individuals are due normally to the germination of spores, and the plants are present on the shore for only a restricted period of the year. Biennials usually produce a thallus in the first year and then reproduce in the second year, though the growth periods are not always seasonal. Perennials live for several years, and reproduce by spores and also by proliferation from certain portions of the thallus. Growth may continue over the whole of the year, or may be restricted to certain seasons. The onset of unfavourable conditions induces reproduction, and in consequence any harvesting of the weeds must be organized with due regard to their reproductive and growth periods.

From early times, seaweed products have been used habitually in the Orient for food, fertilizers, and the making of glue; and later for the extraction of iodine. The peculiar properties of agar were apparently discovered accidentally [1]. A Japanese emperor and his party were marooned in a snowstorm in the mountains, and a peasant family prepared for them a dish of seaweed jelly, the remaining portion of which was thrown out and lay on a shrub, where it froze during the night. When the thaw came, the frozen jelly collapsed, the watery part leaked away, and the remainder shrank to a glistening, crinkled-papery texture of low density. The peasant then discovered that the original dish could be reconstituted by heating the residue with water and allowing it to cool. The incident is said to have been the precursor of the agar industry.

The value of the agar exported from Japan increased enormously in the years from 1869 to 1902 [2]. Twelve thousand tons of weed were harvested in 1936 [3], yielding 2,549 tons of agar, with a value of over 9½ million yen. In the same year, the value of the export was 5½ million yen.

Agar is produced in the form of porous translucent sheets, rectangular blocks, flakes, sheaves of ribbons, or ground to a fine powder. Its colour and quality vary considerably, the best grades being about the colour of gelatine.

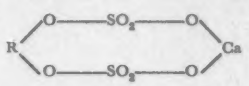
Before the present war, first-grade Japanese agar was made principally from *Gelidium corneum* Lamour and *Acanthopeltis japonica* Okam. Other algae used included *Gelidium subcostatum* Okam., *Gelidium japonicum* (Harv.) Okam., *Gelidium pacificum* Okam., *Campylaephora hypnoides* J. Ag., *Gracilaria confervoides* Grev., and *Pterocladia capillacea*

Born. et Thur. The weed was collected by hand or from boats. The harvest took place from May until the end of July, though manufacture was an activity of the winter months. The collected material was first dried on the shore and allowed to bleach partially in the sun. It could then be stored temporarily. Subsequently it was completely bleached by alternate exposure to sun and moistening with fresh water. The weed then formed mat-like layers which were boiled in water to which dilute acetic acid or still more dilute sulphuric acid was subsequently added. These latter processes are variously interpreted as having facilitated the extraction of the colloids in the cell-walls or as having precipitated the proteins and so made the final product clearer. After filtering, the residue was re-boiled and the total filtrate allowed to set in flat wooden vessels. It was subsequently frozen either in the open air on cold winter nights or in a refrigerator. When the material was thawed, the water flowed away, leaving a relatively pure agar.

The manufactured product has received various names, including agar-agar, kanten, hai thao, agal-agal, Ceylon moss, vegetable fish glue, Japanese or Chinese isinglass, oriental isinglass, agar, or gelose. Agar-agar is the Malayan word for a red alga, *Gracilaria lichenoides* Harv. The term has been used commercially for two things, which has led to confusion in the literature. It is used primarily to mean the trade product which is made from various red algae, but it is also used to describe drugs which are prepared from untreated dried red algae. 'Kanten' means in Japanese 'cold sky,' and refers to the fact that the product was originally manufactured in the cold weather.

The literature concerning the chemistry of agar shows that the results have not been uniform, but it must be recognized that there is great difficulty in getting a uniform product to test in commercial material of this kind. There is known to be seasonal variation in the quality of the gel; the raw material is not uniform; and the manufacturing process depended in part on climatic conditions.

According to Robertson [1], agar has been identified as a galactan or polysaccharide in the molecule of which numerous galactose units are united in a manner suggestive of the grouping of glucose nuclei in cellulose. Small quantities of other sugars complicate the position and probably account for some of the lack of uniformity in analyses. Usually there is 3 per cent. of sulphur present as an organic sulphate, together with a certain amount of calcium and magnesium. The firm combination of all this suggests that the main carbohydrate molecular mass is combined as an ester with sulphuric acid. One of the two acid groups of sulphuric acid is esterified, the second is free to form a calcium salt of the general formula:



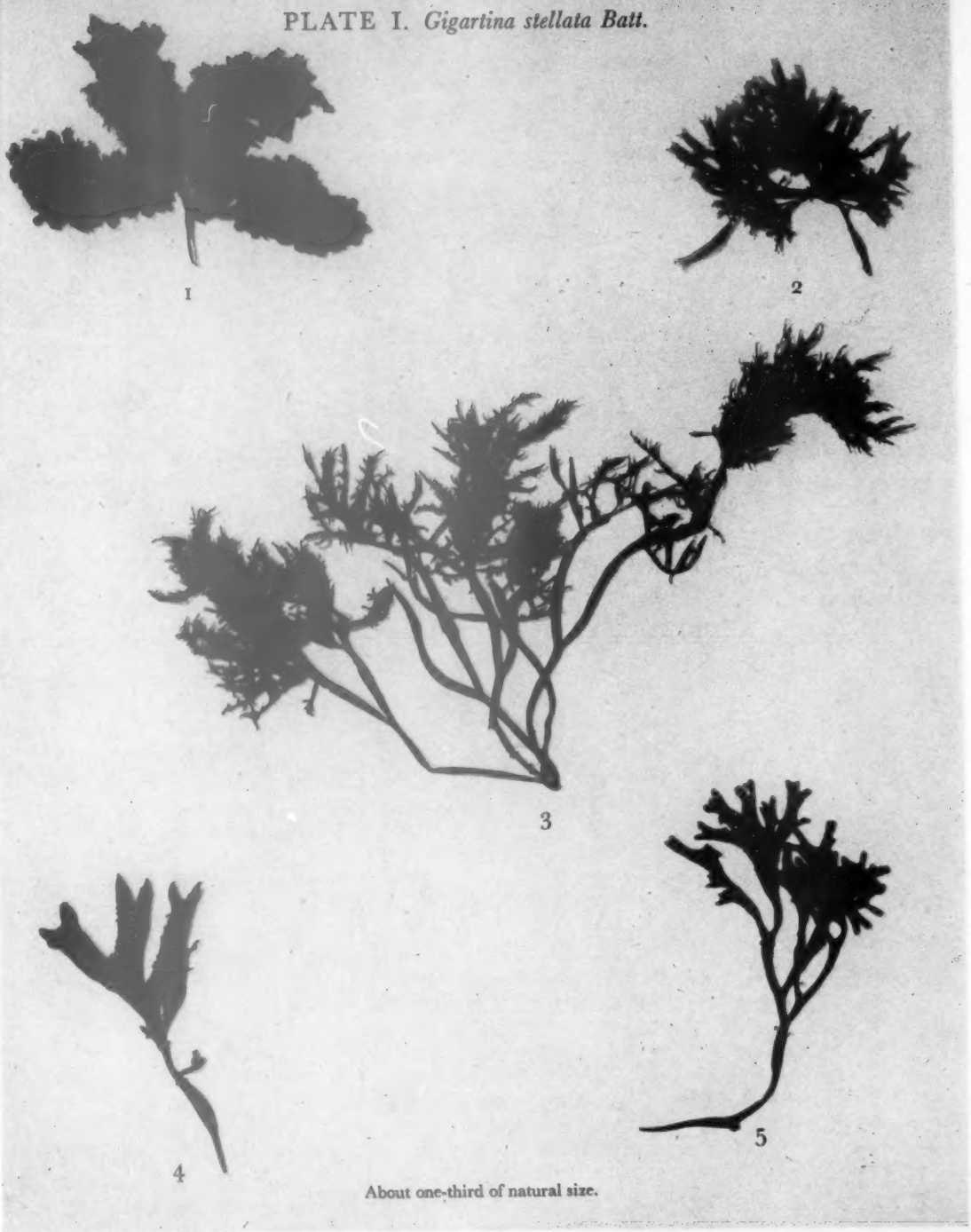
The Japanese practice of using acetic or sulphuric acid in the preparation of agar may cause a little hydrolysis, and so account for the slight inferiority of the Japanese gel as compared with some other varieties.

Jones and Peat [4] consider commercial agar to be the sulphuric acid ester of a linear polygalactose, of which the unit-chain is composed of nine residues of *d*-galactose mutually combined by 1 : 3-glycosidic linkages. This chain is terminated at the reducing end by a residue of *l*-galactose, which is united with the rest of the chain by a glycosidic linkage engaging C_4 and not C_3. The *l*-galactose is esterified at C_6 with sulphuric acid. Barry and Dillon [9] suggest that a 3 : 6 anhydrogalactose linkage exists in the agar molecule. Percival [10] offers an alternative explanation of the results of Jones and Peat and also of Barry and Dillon, in which he suggests that the sulphate group may be on C_3 and not necessarily on C_4.

The physical properties of agar make it invaluable as a medium in bacteriology, into which it was introduced in 1881 by Frau Fannie E. Hesse. It is not liquefied by many organisms which cause liquefaction of a medium containing animal gelatine, and it is not easily ruptured when streaked with an inoculating needle. It remains liquid when cooled to 40° C, and when once solidified remains a gel at blood-heat. It changes readily from sol to gel, or vice versa, when subjected to change of temperature. It forms a firm jelly without refrigeration. Agar is also used in mycology, and recently it has contributed to the research which has led to the large-scale production of penicillin.

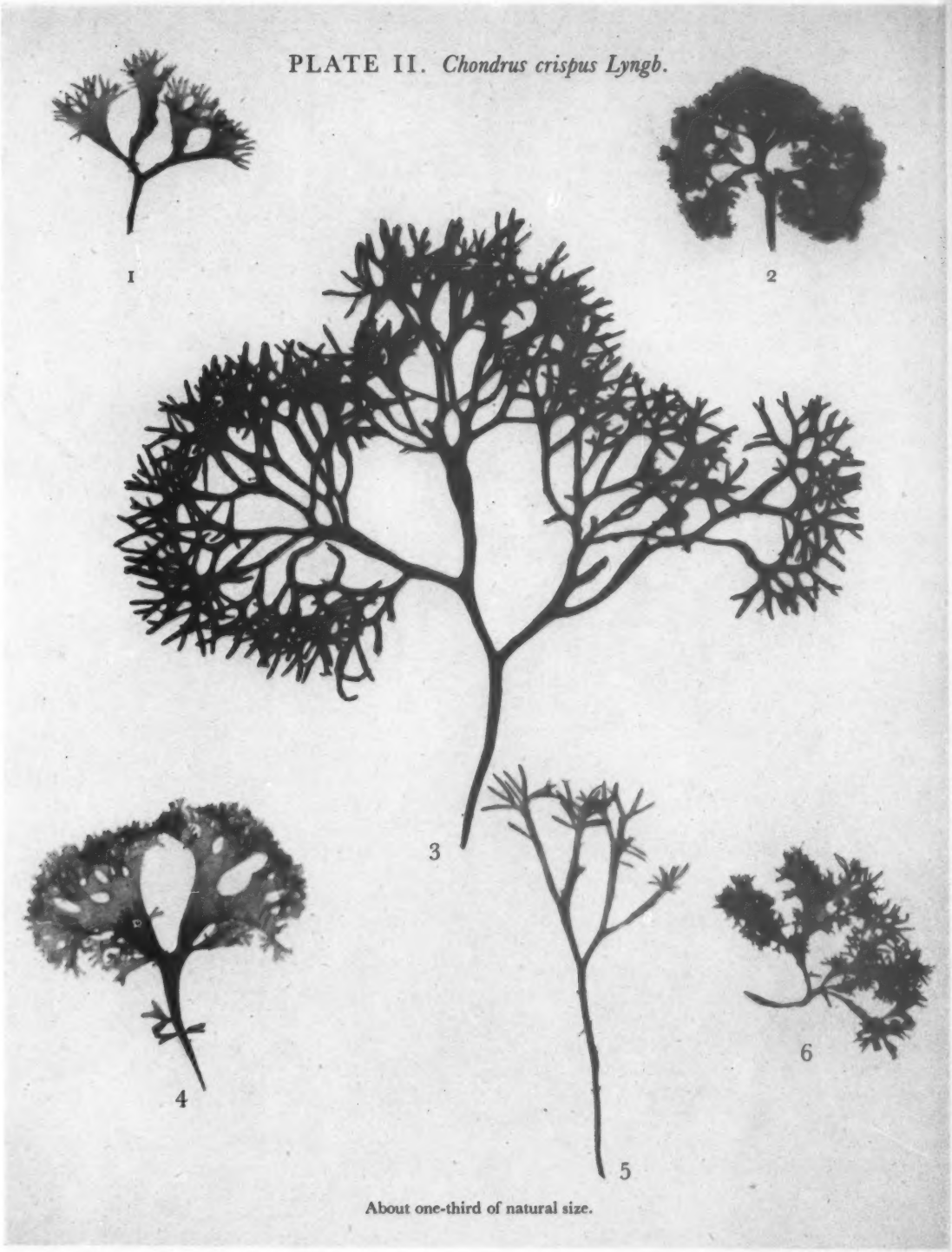
The physical properties of agar, including its colloidal state, have led to its extensive use in medicine and pharmacy. Unlike pectin, it needs no sugar to make it gel. Its use in war-time has been much restricted in order that supplies might be conserved for essential work. In peace-time, however, it was largely used in commerce. It formed a constituent of petroleum-agar emulsion, where its effect was mechanical. The agar does not

PLATE I. *Gigartina stellata* **Batt.**



About one-third of natural size.

1, *Specimen from the Fucus vesiculosus association. August* 1942. *Aberayron, Cardiganshire.* 2, *Specimen from the Fucus vesiculosus association. June* 1942. *Aberarth, Cardiganshire.* 3, *Specimen from the Fucus serratus association. August* 1942. *Llanon, Cardiganshire.* 4, *Specimen from the Fucus serratus association. October* 1943. *Aberdaron, Lleyn Peninsula.* 5, *Specimen from the Gigartina association. April* 1944. *Aberporth, Cardiganshire.* (*This shows the new growth at the thallus tips in spring.*)



1 *and* 5, *Specimens from the Fucus serratus association. August* 1942. *Rhoscolyn, Anglesey.* 2, *Specimen from a shingle pool near high-water mark of spring tides at Aberarth, Cardiganshire. June* 1942. 3, *Specimen from the sublittoral zone. October* 1944. *Aberystwyth, Cardiganshire.* 4, *Specimen from the shingle pools in the inter-tidal belt at Aberayron, Cardiganshire, August* 1944. 6, *Specimen from a rock pool in the inter-tidal belt at Aberystwyth. October* 1942.

remain in the acid medium of the stomach long enough to be hydrolysed, and in the alkaline intestinal fluid no hydrolysis takes place. Agar was used as a pill excipient, ointment base, and glycerol substitute, and as a wound dressing because of its inhibitory action on blood clotting. It contributed to the manufacture of plastic material suitable for making dental imprints, and had numerous uses in the food industry. It formed bulk in the manufacture of diabetic foods and was widely used in the canning of meat and fish —especially the latter, where the firm jelly made a suitable medium in which the soft tissues of cooked fish could be transported unharmed. It was used as a thickening agent in creams, puddings, ice-creams, soups, and meat pies. It was also used as a base in the confectionery trade, as well as in the making of greaseless cosmetics, shoestains, and hand lotions. In the textile trade it formed a sizing medium for fabrics and a thickener in the dyeing and printing of textiles, while in the leather industry it was used in the finishing process.

Agar was a constituent of various high grade adhesives and was used in the manufacture of plywoods. Films prepared from it were used as windows, and photographic films were prepared from its aqueous solution treated with formaldehyde. These needed to be only one-eighth of the thickness of gelatine films. It has been used as an activator in nicotine sprays [5]. An aqueous solution (1 : 500) trebled the effectiveness of the spray for destroying the pea aphid, *Illinoia pisi*. The objection to its general use is the difficulty of dispersing it in cold water, but the use of boiling water to prepare the spray would obviate this handicap. Agar has also been used in the electroplating of lead and the sizing of paper.

The food value of agar to human beings is probably very slight. It appears to be hardly attacked at all by plant enzymes, animal digestive juices, or bacteria. There is some indication, however, of the presence in agar of certain growth-promoting substances. Hoffman [3] records that animals fed on a purely synthetic diet showed faulty growth which disappeared at once when 5 per cent. of purified agar was added. Robbins [6] observed the beneficial effect of agar on spore germination and growth, and also on the formation of gametes and zygotes in the case of *Phycomyces Blakesleeanus*. He found that beneficial material could be extracted from the agar, partially by dilute methyl alcohol and more completely by aqueous pyridine.

In America, a small agar-agar industry has grown up since 1919, *Gelidium cartilagineum* Gaill. being an important constituent together with *Gracilaria confervoides* Grev., *Gelidium corneum* Lamour, *Eucheuma spinosa*, and different species of *Gigartina*. Divers collect the weed by hand-picking and by using long-handled rakes. In 1930, 180 dollars per ton were paid for crude air-dried weed, from which about one-sixth the gross weight was recovered as agar. The problem of labour has always been acute, and Robertson [1] draws attention to the fact that the harvest is 'no job for water-front loafers, but requires hale, hardy, and thoroughly industrious men who know the sea.' Under good working conditions, a man can gather 1,000 lb. of wet weed in a day. The Japanese freezing method has been replaced by controlled refrigeration. The hot agar liquors are filtered under pressure after being treated with charcoal. After freezing, a process of dialysis purifies the agar still further, and the commercial product is of a high grade, though it is said to be inferior in appearance to Kobe No. 1, the best Japanese agar. In 1925, about 360 tons of agar were produced, with a value of over 80,000 dollars. After the attack on Pearl Harbour, the use of agar was restricted by the War Production Board to the preparation of bacteriological media, and subsequently surveys were sponsored for the location of other weeds of possible commercial value. During 1943–44 there was considerable utilization of *Gracilaria confervoides* Grev.

Several countries have increased their agar-production since 1939. In New Zealand a satisfactory product has been made from local species of *Gelidium* and *Pterocladia* [7]. South Africa has used *Gelidium cartilagineum* Gaill., *Gracilaria confervoides* Grev., and *Suhria vittata* J. Ag., with marked success. Other seaweeds have been found suitable for agars of lower grade, but the most profitable source is probably *Gracilaria confervoides* Grev. [8]. Australia had an annual pre-war consumption of 70 tons of agar, which was largely used for canning. During the war, *Gracilaria confervoides* Grev. has been used there and a suitable mechanical harvester devised. This weed grows on a sandy substratum and differs from rock-weeds in its harvesting requirements. Two Sydney firms are producing a satisfactory agar, and there appear to be possibilities that Australia may compete even in a peace-time market.

Considerable research has taken place in Russia in an endeavour to produce agar. *Phyllophora rubens* from the Black Sea is being used, but the

agar is said to require a 6 per cent. solution to form a gel. This is much higher than the normal requirement. According to Kizevetter [11] Russia made about 55 tons of first-class agar from *Ahnfeltia*, in addition to 200 tons of 'agaroid' from *Phyllophora*.

Agar has been made in India and Ceylon from *Gracilaria lichenoides* and *Gracilaria confervoides*, gathered at the time of the south-west monsoons. Ceylon agar is said to contain only 37 per cent. gelose as compared with 60 per cent. in the Japanese product. In Java and Macassar, a small quantity was made from *Eucheuma denticulatum* (Bur.) Collins. China used to import a large quantity of agar from Japan, but is said to have produced a little from *Gracilaria confervoides*.

The west coasts of Ireland have been an important source of *Chondrus crispus* for many years, and considerable research on the ecology of the plant and its commercial harvesting has taken place. The term carrageen is used to mean either *Chondrus crispus* or *Gigartina stellata*, or a mixture of both these weeds. Recent work has led to the conclusion that *Gelidium pulchellum* and *Gelidium latifolium* are suitable plants for the manufacture of a satisfactory agar and that they occur in sufficient quantity on the Irish coasts to make the project profitable. It has been stated [12] that production of agar on a limited scale by one commercial firm began in 1944.

In Britain, the problem of providing a suitable bacteriological agar was recognized at an early stage of current hostilities. Owing to Government foresight, a certain amount of agar had been stored, but it was necessary to ascertain whether additional supplies could be made locally. With this object in view, A. P. Orr and S. M. Marshall of the Scottish Marine Biological Station, Millport, investigated various weeds. It was found that *Gigartina stellata*, or a mixture of that alga with *Chondrus crispus*, after suitable treatment produced an agar the setting-point and hardness of which are highly satisfactory for bacteriological work. It is more transparent when set than the Japanese product, and more resistant to hydrolysis. A coastal survey to locate the necessary seaweed beds has been organized by the Botany Department of the University College of Wales, Aberystwyth, in co-operation with a number of algologists in different parts of the country. In addition, the autecology of the plants is being studied. The harvesting of the weeds has been restricted, and a Government order controls agar or any mucilaginous product made from the plants already mentioned and also from seven other genera. In the case of *Gigartina*, the life-history of the plant is as yet incompletely known, and research work, both cytological and cultural, is in progress.

Very numerous voluntary workers have assisted in the task of harvest. The weeds from which the agar is made, both in Britain and elsewhere, are all relatively small plants, and in Britain the task has meant laborious hand picking. They grow low down on the shore near the low-water mark of ordinary spring tides, so that collecting is restricted by the height of the spring tides as well as by the daily tidal fluctuations. Drying has to be done soon after gathering, and this too is not at all times an easy task in the uncertain British climate.

Another harvesting difficulty arises from the extreme polymorphy of both the plants concerned. The accompanying plates illustrate this phenomenon. In addition, *Chondrus* and *Gigartina* are sometimes so similar in form to each other as to puzzle even an expert. Fortunately it has not been necessary for the collector to distinguish between them, and they have been harvested together.

Much research is still necessary, because judicious harvesting can take place only if the detailed autecology of the plants is fully understood. In addition to laboratory work, such studies entail laborious field work often under rigorous climatic conditions. Despite these difficulties good harvests have been obtained for two years, and a supply of agar is assured.

REFERENCES

[1] Robertson, G. Ross. *Ind. Eng. Chem.*, 1930, **22**.

[2] Tressler, D. K. *Marine Products of Commerce*, New York, 1923.

[3] Hoffmann, Curt. Kieler Meeresforschungen III, 1939.

[4] Jones, W. G. M., and Peat, S. *J. Chem. Soc.*, 1942, **225**.

[5] Sharp, S. S. *J. Econ. Ent.*, 1939.

[6] Robbins, W. J. 'Growth Substances in Agar.' *Amer. J. Bot.*, 1939, **26**.

[7] Moore, L. B. 'The Economic Importance of Seaweeds.' Bull. 85. *D.S.I.R.*, New Zealand, 1941.

[8] Isaac, W. E. *et. al. Nature*, 1943, **151**.

[9] Barry, V. C., and Dillon, T. *Chem. and Ind.*, 1944, **63**, 167.

[10] Percival, E. G. V. *Nature*, 1944, **154**, 673.

[11] Kizevetter. *Bull. Pac. Sci., Fish and Oceanog.*, 1937, 13 Vladivostok.

[12] *Chemical Age*, 1944, **50**, 84.

Particles, waves, and chemical formulae

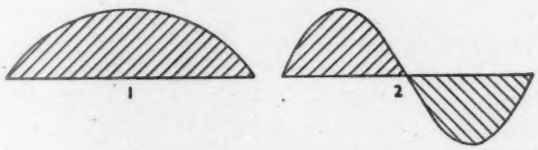
E. J. BOWEN

The fundamental relationship of the elements, as expressed in the Periodic Law, has long awaited satisfactory interpretation. Modern physics shows that the peculiarities of the Law, with its regular valency changes and its periodic repetitions of chemical properties, are found to follow perfectly from the geometry of three-dimensional wave-motion. The theory also opens up possibilities of a quantitative explanation of the relation of colour to chemical constitution, a problem that has both attracted and baffled chemists for some fifty years.

From early times chemists have visualized matter as composed of particles or atoms, and when the electron was discovered it too was regarded as a particle. A single chemical bond between two atoms was found to correspond to a shared pair of electrons, and the molecule of hydrogen, H_2, for example, was pictured as a four-particle system of two positively charged hydrogen nuclei united by two associated electrons forming the link. On the other hand, the behaviour of light rays as observed in refraction, polarization, and interference could be accounted for only by treating light as a form of wave-motion. The particulate structure of matter and the wave theory of light became almost axioms in scientific thought. In the last few decades, however, phenomena were discovered which conflicted with this apparent simplicity. Streams of electrons and of atoms show interference phenomena, when reflected from crystal surfaces, which can be understood only by treating them as waves, while to explain the photo-electric effect it seems necessary to regard light as a stream of particles. This contradictory situation has now been rationalized by viewing both matter and radiation as 'dual personalities,' each having 'particle' and 'wave' properties, and by the recognition that 'particle' and 'wave' concepts are complementary to each other. We have first to clarify our ideas as to what we mean by a particle or a wave. These are abstract concepts derived from notions of real things such as lead shot or sea waves. When we have sufficiently idealized real particles and waves we arrive at the concept of a particle as something possessing momentum mv and energy $\frac{1}{2}mv^2$ (m = mass, v = velocity), while a wave is characterized by a length (wave-length, l) and a time (period of the wave, t). Now since measurements on matter and on light are made by using other matter or light there is an interaction between measurer and measured, so that if we press a measurement to a high degree of accuracy we disturb the system correspondingly in another direction. The principle governing this was discovered by Heisenberg: Δ (*particle attribute*) $\times$ Δ (*wave attribute*) $= h/2\pi = 10^{-27}$ c.g.s. units, where h is Planck's constant and the Δ's signify uncertainties of measurement due to the interaction of the measuring device with the thing measured. The particle attribute *momentum* is associated in the above with the wave attribute *length*; *energy* and *time* are similarly associated.

Let us see the bearing of this principle on matter of atomic dimensions by imagining an electron confined in a box and moving to and fro

ONE DIMENSION



TWO DIMENSIONS

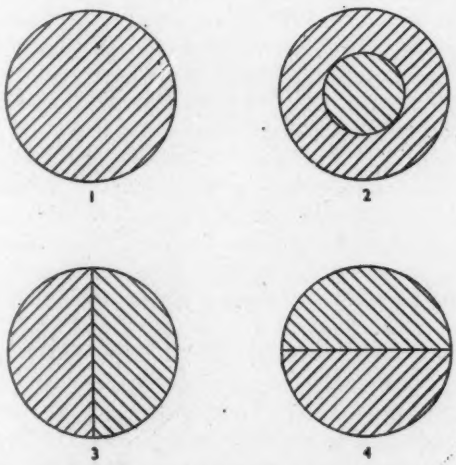


FIGURE 1

between collisions with the walls. If the box is large, we do not specify l very closely; therefore accurate measurements of momentum are possible; all energy values are possible, and the electron clearly has the properties of a 'particle.' In a box so small that its width is a few A only, however, the mere fact that a length is specified so closely means that the momentum becomes vague. It ceases to be practicable to apply 'particle' concepts, and the electron must be regarded as a 'wave.' If its motion is to remain unchanged it must be a 'stationary wave,' i.e. one with fixed nodes (or places of no motion) at the walls. Its energy values are restricted to those given by $\text{energy} = \frac{n^2h^2}{8ma^2}$, where m is the electron's mass, a the width of the box, and n an integer. The lowest energy given by setting $n = 1$ corresponds to a wave with nodes at the walls like that shown for a string stretched between two points in figure 1,1. For $n = 2$ the wave-motion is like the harmonic shown in figure 1,2, and, for higher values of n, waves with an increasing number of nodes between the walls represent the motion. If we inquire more closely into the 'meaning' of this application of wave theory to particle motion we find the following. Because of Heisenberg's principle, we cannot by any physical measurement specify *exactly* where a particle is at any time during its motion; we can only estimate *probabilities* of its being in any particular positions. The 'wave-motion' assumed is the mathematical way of representing these probabilities: the square of the amplitude of the wave at any point gives the probability of the electron being there during any measurement.

There are two fundamental observations which have long awaited interpretation: in physics the quantization of energy and in chemistry the relationship of the elements expressed by the Periodic Law. Wave-mechanics greatly relieves the obscurity of these observations. The quantization of energy, or the fact that the energies of atoms and molecules are confined to certain sets of values, instead of being of any amount, naturally follows from the wave theory. Only stationary waves (waves with fixed nodes) are possible, and these are limited to the fundamental and its higher harmonics. Each 'wave' corresponds to an 'energy state', and intermediate energies do not exist as stable values. The peculiarities of the Periodic Law, with its valency changes across the Table and its repetitions of chemical properties in the series of elements, are found to follow perfectly from the geometry of three-dimensional wave-motion.

For the qualitative application of these ideas to atoms and molecules it is first necessary to form some notion of wave-motion in space. The one-dimensional vibration of a string in figure 1 is illustrated by the fundamental and the first harmonic (shading represents opposite phases of vibration). In two dimensions we have the motion of a drum membrane. The first diagram shows the fundamental and the second a first harmonic with a circular node, the centre portion moving up while the outer region is moving down. Diagrams 3 and 4 show two other first harmonics of a different type, having a linear node, one half of the membrane moving up while the other half is

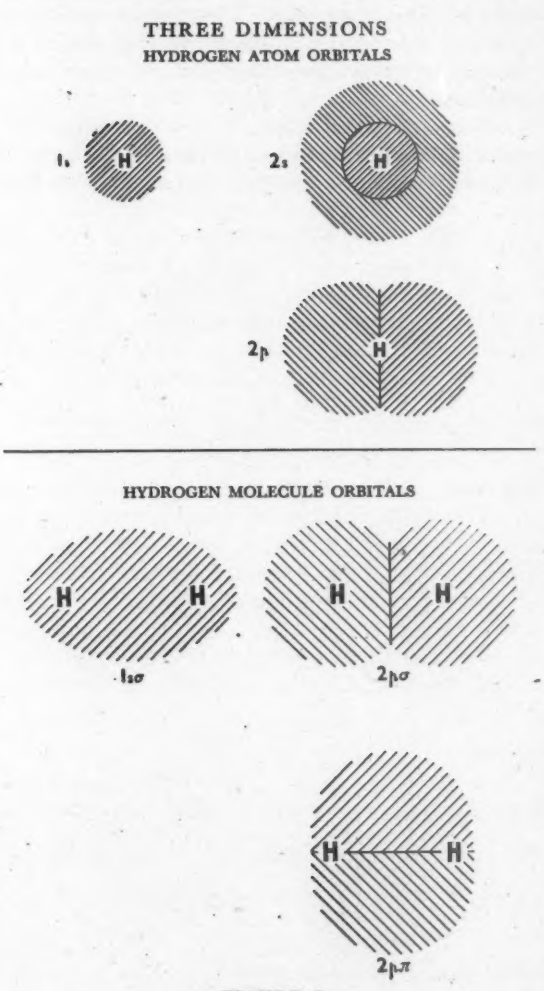


FIGURE 2

moving down. These two types of harmonic prepare one for the three-dimensional waves representing the electron of a hydrogen atom (figure 2). The fundamental or lowest energy state is a spherical vibration similar to rapid compressions and expansions in the neighbourhood of a point in an elastic solid. The only node is at infinity, but the amplitude dies away practically to zero at a short distance from the nucleus of the atom. The electron is designated 1*s*, 1 giving the number of nodes and *s* standing for the spherical symmetry of the wave. Since the square of the amplitude is also of spherical shape, the electron has an equal probability of being found in any direction at a chosen distance from the nucleus. It cannot be localized more closely than this. A higher energy state is the harmonic 2*s*, with a spherical node separating regions of opposite phase. Corresponding to the types of motion 3 and 4 of the drum there are three 2*p* vibrations, at right-angles to each other, each with a planar node separating opposite vibrations. Here the electron may be found on either side of the nodal plane, but not in the plane.

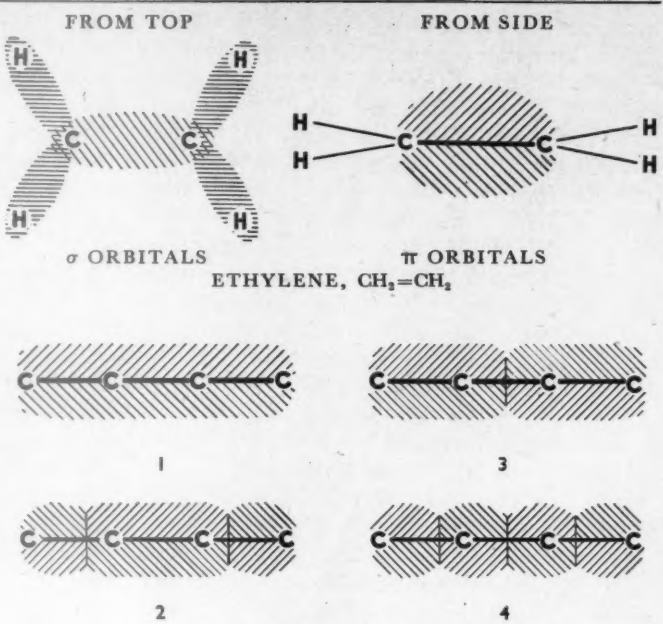


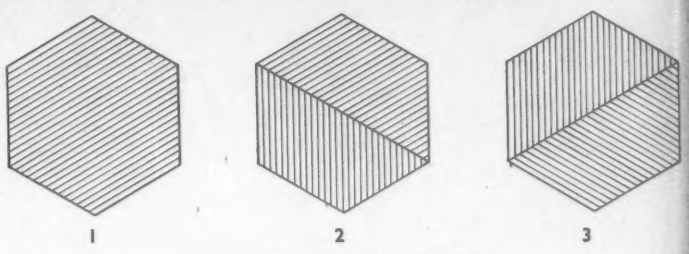
FIGURE 3

These types of vibration take the place of the 'orbits' originally postulated by Bohr, and are consequently by analogy called 'orbitals.' A fundamental principle due to Pauli governs the assignment of electrons to orbitals. Experiment shows that electron-waves have a kind of magnetic polarization called the 'spin.' We may regard the atomic nucleus with its force-field as being surrounded by potential orbitals, in the same way that an unstruck bell may be said to possess potential tones and overtones. According to the Pauli principle we can have up to two electrons in any orbital, but if there are two, their 'spins' must be opposed. The helium atom has orbitals similar to the hydrogen atom; in its lowest energy state it has two electrons of paired spin in a 1*s* orbital. The paired spin explains its lack of chemical reactivity. The lithium atom in its normal state has two electrons of paired spin in the 1*s* orbital, while the third must have its behaviour represented by the 2*s* orbital. Later elements have electrons in the three 2*p* orbitals, until we have two electrons in each of the 2*s* and 2*p*, making eight in all, in the shell of quantum number 2, and we arrive at neon. The next element, sodium, must have its additional electron in the 3*s* orbital and is therefore spectroscopically and chemically similar to lithium. Higher in the Table the complexities of the transitional elements and the rare-earth metals can be easily comprehended by means of the more elaborate geometry of the upper harmonics of the wave-motion.

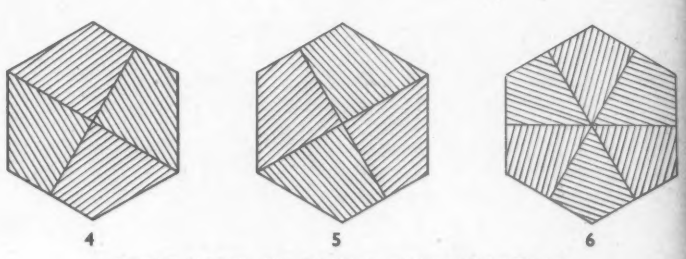
The 'shapes' of the 'orbitals' of molecules, and hence (by squaring) the 'electron density' curves, are quantitatively extremely difficult to calculate with precision, but their general nature may be quite easily seen from symmetry considerations. The orbital of lowest energy of the diatomic molecule H_2 has a somewhat ellipsoidal shape which is symmetrical round the molecular axis (figure 2), and is denoted 1*s*σ or σ_g because of its resemblance to the 1*s* orbital of the H atom. Higher orbitals to which an electron goes when the molecule is given electronic energy are 2*p*σ or σ_u, with a nodal plane at right angles to the molecular axis, and 2*p*π or π_u with a nodal plane in the molecular axis (figure 2). It is necessary to visualize the peculiar features of these orbitals to understand the extension of wave-conceptions to organic chemistry. A σ_g orbital is sausage-shaped along the molecular axis, and corresponds to the ordinary chemical bond or single link H–H, or in

'particle' electronic notation (Lewis-Langmuir), H : H. In the σ_u orbital the two halves of the molecule are out of phase with each other; one or two electrons occupying such an orbital are 'anti-bonding,' i.e. bring about repulsion rather than attraction between the atomic nuclei. The π_u orbital is shaped like a penny bun sliced across for buttering. Each half is of opposite phase and the butter layer is a nodal plane in which the atomic nuclei lie. A further type of orbital, π_g, has another nodal plane at right-angles to the molecular axis, and is therefore an 'anti-bonding' orbital like σ_u.

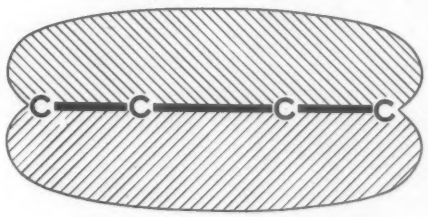
We are now in a position to attack the normal quadrivalent carbon atom. This atom has six electrons. Two (spin-paired) occupy a 1*s* orbital. The remaining four must occupy higher and larger orbitals. If one electron is assigned to the 2*s* orbital and to each of the mutually perpendicular 2*p* orbitals, we obtain an atom with four 'free valencies,' but not all equal or tetrahedrally arranged. Consider, however, the interaction of this atom with four 1*s* hydrogen atoms to give the methane molecule. One hydrogen atom can unite with the 2*s* electron of the carbon to give a σ_g type orbital, but it is easily seen that the combination of the remaining three 1*s* hydrogen atoms with the 2*p* electrons of the carbon cannot be simple. If the hydrogen part of the orbital is in phase with one half of the carbon part it must be out of phase with the other, and the link will be partly σ_g (bonding) and partly σ_u (anti-bonding). Approximate wave-theory calculations show that a system of the *lowest energy* is produced by a radical rearrangement of the vibrations by which all the C–H links become equivalent and *tetrahedrally* disposed, the orbitals of the links being essentially σ_g (sausage-shaped and directed along C–H) in character, with a trace of σ_u (represented by a small area of opposite phase on the C atom away from the hydrogen). This type of orbital of essentially σ_g character represents the 'single bond' of organic chemistry as in methane or the diamond.



FLAT VIEWS OF RINGS



EDGE-VIEW OF RINGS FOR ORBITALS



π ORBITALS OF BENZENE, C_6H_6

FIGURE 4

The structure of graphite as determined by X-rays introduces us to a different rearrangement of orbitals when a carbon atom is combined with other atoms. Here we find three single links (of σ_g type) in a plane at 120° to each other, and the remaining electron of each carbon atom is essentially a *p* orbital with its nodal plane forming the plane of the single links. These less strongly bound *p* electrons have orbitals which overlap in the spaces between the sheets of carbon atoms in graphite to form a π orbital, and account for the easy cleavage, high electrical conductivity, and powerful light-absorption, which are all of a highly directive nature in the planes between the sheets. Chemists now believe that a 'double bond' in organic chemistry is not two single bonds but one bond of σ_g type and one π type bond. These two kinds of orbital are shown for ethylene in figure 3. All the C–H bonds and one C–C bond lie in one plane at 120° to each other, and this plane is

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the nodal plane of the π orbital of the second C–C link. The two electrons in the π orbital exist in two regions above and below this plane, and prevent 'rotation about the double bond.' In the normal ethylene molecule both electrons are in a π_u orbital, while in the molecule excited by ultra-violet light one electron passes to a π_g orbital, weakening the link.

Some orbitals of butadiene are also shown in figure 3. There are four electrons left over after the 'single bond' skeleton of the molecule is built up, and these must be fitted into π type orbitals with a nodal plane containing the carbon atoms. Orbitals 1 and 2 have the lowest energy; hence in the normal molecule one pair of electrons (spin-paired) has its spatial distribution represented by the square of the 'vibrations' depicted in orbital 1, and the second pair (also spin-paired) is represented in a similar way by orbital 2. This explains the 'conjugated' nature of the molecule, i.e. its low degree of unsaturation and the partial 'double bond' character of the central C–C link. The wave representation differs from the chemical formula $CH_2 = CH - CH = CH_2$ in the assumption that the π electrons move in orbitals which may extend over more than one C–C link. For this to be possible a molecule must have alternate double and single bonds.

Figure 4 shows the π orbitals of the benzene molecule. The electrons of the 'aromatic sextet' occupy in pairs the orbitals 1, 2, and 3. The distribution of these electrons (square of wave vibration) is in the form of a cloud on each side of the carbon ring, sandwiching between them the σ C–C bonds. Orbitals 2 and 3 have an additional nodal plane besides that of the ring. The total 'electron density' of the π electrons obtained by summing those of the three electron pairs has perfect hexagonal symmetry.

The mathematical treatment of atomic and molecular orbitals is exceedingly difficult except in the most simplified cases. There is a fundamental differential equation forming the basis of quantitative wave-mechanics which cannot be rigorously solved except for the systems H, He, and H_2. The difficulty in other cases lies in the complexity of the force-fields holding the electrons to the nuclei. Tentative advance is made by a series of drastic approximations. Atomic orbitals of an atom such as carbon are calculated as if they were hydrogen orbitals with a different nuclear charge. Molecular orbitals are approximated to by linearly combining atomic orbitals in such a way as to make the energy a minimum; and π orbitals of butadiene or benzene, which are not localized at particular links, are approximated to in the same way by linearly combining sets of localized molecular orbitals with minimization of the energy. In this way estimates of the energies of the molecule may be made. Another mode of approximation developed by Pauling makes use of the artificial concept of 'resonance between valency structures.' The energies of higher orbitals, to which electrons pass only when the molecule absorbs visible or ultra-violet light, such as orbitals 3 and 4 for butadiene (figure 3) and 4, 5, and 6 for benzene (figure 4), may also be derived with some degree of accuracy, and so permit of estimates of the position in the spectrum of molecular absorption bands. Such calculations have been made by Sklar, Mulliken, and others for quite complex molecules such as butadiene, aromatic hydrocarbons, carotene, and certain dyes, and place the theory of colour and constitution on a numerical basis for the first time.

It has already been mentioned that wave-mechanics assumes for graphite two-dimensional layers of 'non-localized' electrons between the atom sheets. For solid metals like sodium the valency electrons are assumed to be non-localized in all three dimensions, and an interpretation is thus reached of the typical metal: electrically conducting, very opaque to light, and showing other properties of mobile electrons such as thermo-electric power.

REFERENCES

LENNARD-JONES and COULSON. *Trans. Faraday Soc.*, 1939, **35**, 811.

COULSON. *Proc. roy. soc. Edinb.*, 1942, 115.

HÜCKEL. *Z. Elektrochem.*, 1937, **43**, 752, 827.

FORSTER. *Ibid.*, 1939, **45**, 548.

Reports on Progress in Physics, 1941, **8**, 231.

Review of Modern Physics, 1941, **13**, 75; 1942, **14**, 224–340.

Annual Reports of the Chemical Society, 1943, **40**, 12.

Book reviews

THE ROYAL SOCIETY OF LONDON

The Royal Society, 1660–1940. A History of its Administration and its Charters, *by Sir Henry Lyons. Pp. x + 354. University Press, Cambridge.* 1944. 25*s. net.*

In England, in the first half of the seventeenth century, the new or experimental philosophy was applied by a small group of knowledgeable men to inquiries into a variety of natural things and events, as had been advocated by Francis Bacon. On the restoration of the monarchy in 1660, this group resolved to constitute a Society of Philosophers, and early in the following year King Charles II gave permission for the project to be designated 'The Royal Society.' This led to the incorporation of the Society by Royal Charter in July 1662. Ten months later, an amended Charter granting further privileges passed the Great Seal and is that by which the Society has since been governed.

The grounds and intention of the Letters Patent thus granted are stated in the opening clause to be the encouragement of philosophical studies, 'especially those which by actual experiments attempt either to shape out a new philosophy or to perfect the old.' In the Second Charter the title of the Society was changed from that of 'The Royal Society' to 'The Royal Society of London for improving Natural Knowledge.' The extension of the title was deliberate, in order to emphasize that observations and experiments were to be made with the object of discovering and verifying natural truths independently of supernatural intervention or superstitious beliefs. This is expressed in the motto of the Society, *Nullius in Verba*, which was suggested by the diarist and author, John Evelyn, and is an abbreviation of a line of the poet Horace, *Nullius addictus iurare in verba magistri.* It signifies 'Not bound by the words of an authority,' and pronounces a separation of the freedom of scientific inquiry from scholastic tradition.

Sir Henry Lyons describes the development of the Royal Society throughout its existence, with particular attention to its administrative biography. From its foundation until 1830, only one-third of the Fellows of the Society could be classified as engaged in studies relating to the natural sciences, the other two-thirds having been elected for social or financial reasons. A chapter is devoted to 'The Scientific Revolt: 1820–60,' when a change of policy was instituted, so that by 1860 the scientific Fellows were more numerous than their colleagues for the first time since the Society was founded. This marked the beginning of a new epoch in the history of the advancement of natural knowledge, through the activities of the most highly renowned scientific society in the world.

Sir Henry Lyons made his draft of this story in January 1940. He was able to complete the work to the final passing of the proofs, but his death in August 1944, before the volume was published, deprived his many friends from expressing to him their deep appreciation of this detailed record of the affairs of the Society to which he gave many years of devoted service.

R. A. GREGORY

ASTRONOMY FOR THE ARITHMETICALLY MINDED

From Atoms to Stars, *by Martin Davidson. Pp.* 188, *with* 55 *figures. Hutchinson's Scientific and Technical Publications, London.* 1944. 15*s. net.*

A knowledge of modern views about the structure of the atom and the nature of nuclear reactions is essential for the understanding of the processes by which the energy radiated by the stars is generated. This book, which is intended to provide an introductory outline of present day knowledge of the heavenly bodies, therefore begins with a chapter on the structure of the atom, including an account of isotopes and atomic disintegration. The author has aimed throughout at showing how various results about the distances, sizes, masses, temperatures, etc., of the planets and stars are derived. The knowledge of mathematics required is very elementary; there are few formulae but many figures. In some places, rather fewer figures and more formulae would have been preferable, for a mass of figures can conceal a simple principle. However, there are many readers who will find it of interest to see how the actual numerical values come out; it will remove the feeling, which many people appear to have, that there is something mysterious involved in finding, for instance, the mass of a planet or the size of a star. The book is up-to-date and generally accurate, and can be strongly recommended. It contains one bad misstatement—that the double tides at Southampton are caused by one tidal wave proceeding up the Solent, and a second wave passing round the outside of the Isle of Wight and up through Spithead. This erroneous statement has been so often made and so often refuted that it is time that it disappeared from the textbooks.

H. SPENCER JONES

BRITISH FLOWERS

Flowers in Britain, *by L. J. F. Brimble. Pp. x + 393, with* 18 *coloured plates and* 167 *text illustrations. Macmillan & Company Limited, London.* 1944. 12*s.* 6*d. net.*

Though this book is intended for the general public, and is not a textbook of botany, it is one which all botanists will wish to possess. Mr Brimble knows and loves the flowers of Britain; he writes in effortless style; and he has lavishly illustrated his text with excellent photographs. The book begins with a section on the structure and classification of flowering plants, nicely judged to inform but not weary the uninitiated: this was a wise preamble, for it enables the author to give character to what follows. In the main portion of the book, flowers are considered according to their botanical families, each family being divided into those plants which are indigenous to Britain and those which are cultivated for their beauty or economic use. To make the story more complete, plants neither indigenous to, nor cultivated in, Britain are mentioned when they or their products are familiar.

From an apparently inexhaustible mine of information, Mr Brimble has selected the most interesting facts, so that, once taken up, the book is difficult to set down. Quite apart from the simple botanical descriptions, there are references to medicinal and other uses, much about the folk-lore of flowers and the etymology of plant names, and very numerous quotations serving not only to show the literary importance of plants but to enhance the attractiveness of the narrative. Mr Brimble is to be congratulated and thanked; and so are his publishers, who, in spite of present difficulties, have given a fine book a worthy format.

Notes on contributors

F. C. BARTLETT,
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Was born in 1886 and was educated privately and at St. John's College, Cambridge. In 1914 he became Assistant to the Director of the Psychological Laboratory, Cambridge, and eight years later was appointed Reader in Experimental Psychology in the University. Since 1931 he has been Professor of Experimental Psychology there, and is also Director of the Psychological Laboratory. In 1941 he was elected to the Medical Research Council. He has edited the British Journal of Psychology for over twenty years, and has written many books and papers on psychological subjects. He was President of Section J of the British Association for the Advancement of Science in 1929, and is a Foreign Associate Member of the *Société Française de Psychologie*. Among his recreations are cricket, lawn tennis, and walking.

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Was born in 1871 and was educated at the Oratory School, Edgbaston, and Trinity College, Cambridge. He is a Fellow of the Royal Meteorological Society (of which he has twice been President), of the Royal Astronomical Society, and of the Royal Photographic Society. Long interested in meteorological observations, he developed the use of sounding balloons, and was the first in Britain to send a balloon into the stratosphere. During the war of 1914–18 he joined the Meteorological Office, and later formed and commanded the Meteorological Section (Home) of the Royal Engineers. He is a Member of the International Commissions for the Investigation of the Upper Atmosphere and for the Study of Clouds. His principal recreations are sailing and photography.

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Was born in Monmouthshire in 1896, though he is a Scotsman by descent. After some years in the Regular Army, in which he served in the 24th Regiment (The South Wales Borderers), he entered University College, London, in 1920 and carried out research under Professor W. E. Garner on the heats of adsorption of gases by charcoal. Later he turned from physical chemistry to the history of chemistry, and is now Lecturer in the Department of the History of Science in the University of London. He has written many papers on the history of chemistry, and in 1935 published an authoritative biography of Lavoisier. His *Discovery of Specific and Latent Heats* (in collaboration with N. Heathcote) established the priority of Joseph Black's work in this field. He is one of the founders and editors of *Annals of Science*, a quarterly journal dealing with the history of science in the modern period.

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Was born at Watford in 1896 and was educated at Coopers' Company's School and University College, London. In peace-time he is the Senior Lecturer in Physics at King's College, Newcastle-upon-Tyne (University of Durham). He served in the R.A.F. in the war of 1914–18, and during the present one has been engaged on research in acoustics and hydrodynamics for the Services. He has specialized in the physics of sound—a subject to which he was drawn by his interest in music—and has written many books and papers on acoustics as well as on the general applications of physics.

LILY NEWTON (*née* BATTEN),
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Was born in 1893, and was educated at Colston's Girls' School, Bristol, and the University of Bristol. After graduating, she joined the staff of the Botany Department of the University, whence she subsequently proceeded to a Lectureship at Birkbeck College, University of London. Professor Newton later carried out research at the Imperial College of Science, London, under Professor V. H. Blackman, and prepared for the British Museum (Natural History) a *Handbook of British Seaweeds*. In 1925 she married W. C. F. Newton, cytologist at the John Innes Horticultural Station, and on his death in 1927 was appointed to the staff of the Station. In 1928 she returned to teaching, and in 1930 was elected to the chair of botany at the University College of North Wales, Aberystwyth. Professor Newton is much interested in the future position of pure and applied science and the effects which the closer links between the two will have on the post-war training of scientists and teachers. Her professional occupation with plants is supplemented by a fondness for gardening.

E. J. BOWEN,
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Was born at Worcester in 1898. He went to Balliol College, Oxford, as a Brackenbury Scholar in 1915, and afterwards served with the Royal Artillery in France in 1917–18. Returning after the war, he took his degree in 1920 and became a Demonstrator in the physical chemistry laboratories at Balliol and Trinity Colleges, Oxford, under Sir Harold Hartley. He was elected a Fellow of University College, Oxford, in 1922, and of the Royal Society in 1935. From 1935 to 1936 he served as Junior Proctor of the University, having previously acted for some time as Domestic Bursar of his College. He is now University Demonstrator in Chemistry in the newly built Physical Chemistry Laboratory at Oxford (Lord Nuffield's foundation), and a member of numerous University boards and committees. His chief interests in research work have been in the fields of photo-chemistry and fluorescence. His book entitled *The Chemical Aspects of Light* was reviewed in ENDEAVOUR in October 1942.

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